THE
ASTROPHYSICAL JOURNAL
AN INTERNATIONAL REVIEW OF SPECTROSCOPY
AND ASTRONOMICAL PHYSICS

VOLUME XXVI JULY 1907 NUMBER 1

THE OPTICAL AND PSYCHOLOGICAL PRINCIPLES INVOLVED IN THE INTERPRETATION OF THE SO-CALLED CANALS OF *MARS*

By SIMON NEWCOMB

The features of the planet *Mars* as described by Schiaparelli, Lowell, and other observers are so remarkable that the question of their interpretation is of great interest. The divergence between the descriptions and delineations of these features emanating from different observers is well known, and seems scarcely normal. Accepting, as we should, the general principle that what is seen by a single practiced observer under the most favorable conditions affords evidence which completely outweighs that of less favored observers, we must still admit that absolute inconsistency between the two should not be found. The details successively added under improving conditions may not be inconsistent with a rational interpretation of what was previously seen. It has been emphasized by Williams and also by Lowell that an observer with the widest experience in seeing only one class of features may fail in seeing those of another class. Even if this contention is correct, it does not seem to afford a completely satisfactory view of the case.

The optical and psychological principles involved in the interpre-

tation have been investigated and set forth by Lowell in his publications and researches relating to *Mars*. But it seems to me that there still remains something to be done in this direction; and the present paper is the outcome of an attempt to investigate the optical and psychological causes on which depends the judgment of an observer in scrutinizing faint and difficult features on the surface of a planet. While the writer cannot pretend to have solved all the difficulties of interpretation, and may have brought out more than he has solved, he hopes to do something toward laying a basis for further progress.

Two sets of principles come into play, one optical, the other psychological. Under the first head are included all the causes which affect the formation of an image on the retina of the eye; under the second the causes which affect the observer's perception of this image. The principal agencies which come into play in the first class are the atmosphere, the instrument, and the eye. What I have done relates mainly to the instrument, the aberration of the eye being well understood, and involving no principles not at play in the instrument.

A. OPTICAL PRINCIPLES

Notwithstanding the great volume of literature relating to the telescope, I am unable to refer to any one source where the effects of the secondary aberration of an achromatic lens, and the primary aberration of the eye, are stated in a form to be easily applied to this special question. The complete development of the subject would be foreign to the purpose of the present paper, but it seems necessary to summarize the data and formulae in such a way that they can be readily applied. To do this I use the following notation for the quantities which come into play in the working of the ordinary two-lens achromatic objective:

p and p', the geometric power of the objective, for which I take the sum of the reciprocals of the radii of curvature. The accented quantities throughout are those which relate to the flint lens.

k, the ratio $p' \div p$ of the geometric power of the flint, taken as positive, to that of the crown.

$\nu = \mu - 1$, μ being the index of refraction.

a, linear radius of aperture of objective.

f, focal length of objective for any ray. Since f is in general

different for different rays, we assume a special value l for the focus to which the eyepiece is adjusted for sharp vision, and put

$$l-f=r\,.$$

Passing the focal plane P through this adopted focus, r is then the distance of the true focus of any ray from the plane P.

ρ, linear radius of the circle of aberration formed by the light of any ray converging from the objective upon the plane P. For a ray coming to a focus on P we shall have $\rho=0$.

s, the angular radius of the circle of aberration upon P, as seen from the objective.

It may be remarked that a rigorous determination of the focal length and of the spherical aberration is unnecessary. We assume the latter to be perfectly corrected, and an approximate numerical value of the same focal length is sufficient for our purpose.

The focal length for any ray is given by the equation

$$\frac{1}{f}=vp+v'p'=p(v-kv')\,. \tag{1}$$

We may call k the achromatizing factor; p and q are constants for the telescope, and k is so taken that the focal length shall be the same for some two rays, which we may designate by the suffixes 1 and 2. We shall then have, to determine k,

$$kv'_1-v_1=kv'_2-v_2\,,$$

which gives

$$k=\frac{v_1-v_2}{v'_1-v'_2}. \tag{2}$$

The exact value of k, like that of p and q, is a function of the curvatures of the lenses. Regarding it as given, the focal length for each separate ray is determined by the equation (1).

The linear radius of the circle of aberration upon the plane P is given by the equation

$$\frac{\rho}{a}=\frac{r}{f}=pr(v-kv')\,, \tag{3}$$

and the corresponding angular value s of this radius is given by

$$s=\frac{\rho}{l}=\frac{a}{l}\frac{r}{f}\,. \tag{4}$$

From this equation we may compute the value s for the different rays in any telescope for which the constants p and q are given, the in-

dexes of refraction of the glasses being also supposed known. All accurate determinations of the indexes for crown and flint glass as ordinarily used show that the properties of these two glasses are so related that for a given ratio of focal length to aperture, and for a given chromatic correction, the values of s are practically the same for all combinations of such glasses hitherto investigated. Of course, the special kinds of glass made by the well-known Jena manufacturers are here to be excepted; but such glasses have not yet been extensively applied in observations on the planets. It is therefore not necessary to inquire into the special properties of the glasses used in this or that telescope. The indexes of any combination of flint and crown will answer the present purpose. We take for our purpose the objective of the Mount Hamilton 36-inch equatorial, the refractions of which were very carefully determined by Professor Charles S. Hastings. The results which we have used are shown in the table next following.

I do not know the precise value of the achromatizing factor used in the construction of this telescope, nor is it important for our purpose to know it. It will suffice to use a factor having as nearly as may be the best value for the visual field. This value cannot be definitely fixed, because from the brightest part of the spectrum the luminosity fades off more rapidly toward the red than toward the blue. It follows that the combination which will bring the greater part of the light into the smallest space is not exactly that which will produce the brightest stellar image at the central point. I have assumed $k=0.508$ to be as good as any, but our conclusions will not be altered by any admissible change in this factor.

The first column of the table designates the rays which Professor Hastings measured. The first two columns of numbers give ν and ν' for the rays in the brightest part of the spectrum from C to F. The third of these has no designation other than its wave-length, which is 5614.

We next have $\nu - k\nu'$, which is the inverse focal length in terms of the inverse geometric power of the crown lens. The maximum value of this quantity falls, as it should, between D and the ray next below it, and is not far from 0.194636. This would be the focal setting to secure the sharpest central image of a star. But, practically, a better focus to secure the minimum dispersion will probably correspond to

the number 0.194630. Taking this value, we have, by subtraction, the values of

$$\Delta = \frac{1}{l} - \frac{1}{l+r} = \frac{r}{lf},$$

found in the fourth column of numbers.

We have to derive from Δ an expression for the radius of the circle of aberration. Comparing (4) and (5), and putting $\frac{1}{l} = 0.1946$, we find

$$s = 5.14 \frac{a}{l} \Delta .$$

Since s contains as a factor the ratio of the semi-aperture of the telescope to the focal length, it will, in a given telescope, be proportional to the breadth of the aperture actually used, and may be reduced indefinitely by diminishing this aperture. But apart from the drawback of cutting off the light, reduction of the aperture increases the defect arising from diffraction, of which we have taken no account in the preceding geometrical derivation. It is not likely that any result can be secured better than the one we should obtain by assuming a ratio of aperture to focal length of 1:20, which gives $s = 0.1280$. Multiplying by the seconds in radius, we find that the minimum value of s which we can reasonably expect to secure may be expressed with all necessary precision by the formula

$$s = 26400'' \Delta .$$

The values of s thus derived are given in the last column of the table.

Ray	ν	ν'	$\frac{1}{f}$	Δ	s
C.....	0.511565	0.624043	0.194551	− 79÷10	2.0
D.....	.514164	.628994	.194635	+ 5÷10	0.12
λ5614.....	.515474	.631578	.194632	+ 2÷10	0.05
E.....	.517440	.635476	.194618	− 12÷10	0.30
F.....	.520316	.641340	.194515	−115÷10	2.9

Without going into details of computation, it follows from these numbers that by no focal setting can all the light contained between the second and third lines, or between wave-lengths λ 5894 and λ 5614, a range of 280 tenth-meters, be brought within an aberration circle the radius of which is much less than 0".10, or the diameter much less than 0".20. If there is a possibility of the image being made materially smaller, we may regard it as certain that the effect of

atmospheric softening and of diffraction will be to enlarge the image beyond this radius. If we set the focus so as to secure a brighter central point, we increase the radius for the rays on each side, and vice versa. The range of wave-lengths between the lines C and E is 0.1290, and extends from the dark red to the fainter portion of the green, excluding the blue entirely. Were all the light outside these limits brought in and distributed so as to form a uniform spectrum, I conceive that it would be as bright as if the light of the brighter portion were spread between the limits C and E. I therefore conclude that *in using the best refracting telescope under the best conditions we cannot expect to bring more than one-fourth the light within a circle of radius 0".10, three-fourths being distributed outside this circle.*

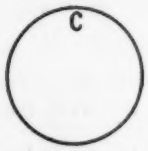


FIG. 1.

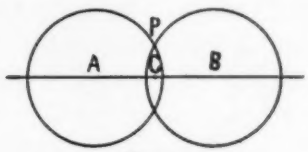


FIG. 2.

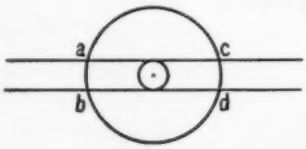


FIG. 3.

The injurious effect of this dispersed light may be lessened by the use of a suitable absorbing screen, which is among the devices used at the Lowell Observatory. I do not know how successful this device has proved in bettering the definition, but it seems quite certain that it could not be so applied to bring the bright central image within the limit of 0".10, and, in fact, would have to be wonderfully adapted to the requirements to bring it even to this limit.

We have next to consider the respective effects of this aberration upon a star and upon a line. In the former case the light of each ray being spread into a circle of radius s, the surface brilliancy of the disk produced by the light of any ray is inversely proportional to s. It therefore rapidly diminishes as we go out from the central point, which accounts for the fact of a companion star being visible at a distance of less than 0".1 from the central star.

A study of Figs. 1, 2, and 3 will show that when a line is observed

the result is materially different. In Fig. 1 let the central point be that of a stellar image, and let the circle be that of aberration for a ray of any wave-length. Then, as just stated, the intensity of the illumination by this ray over the entire surface will be proportional to the inverse square of the radius, and directly as the intensity of the ray. Since, as we approach the center of intensity, the light increases, while the radius diminishes, the illumination increases with greater and greater rapidity, so that the central point is distinctly marked.

In Fig. 2 let the horizontal line represent a line of light on the focal plane, as it would be if perfectly sharp. Let us next inquire into the amount of the illumination at a point P, at a perpendicular distance C from the line as produced by aberration. Take the points A and B at such a distance that $AP=PE=S$, and draw a circle around A and B through P. The point P, instead of being dark, will then be illuminated by all the C-light emanating from the segment AB of the line. Without going over the process of geometric and algebraic reasoning which would be necessary to give the conclusion a precise form, it will be clear enough that the illumination at P would be proportionately brighter than if the light were dispersed only from the center C.

In Fig. 3 let the lines AC and BD represent a perfectly dark strip observed on a bright background. From any point in the central line of the strip describe an aberration circle tangent to the two boundaries. It is evident that this point, and therefore the entire central line, will be black only with respect to the rays, say R, whose radius S of aberration is less than half the breadth of the line. For, to fix the ideas, let the breadth be $0''.20$. Then in the case of the hypothetical telescope adjustment which we have assumed, the central line would be darkened only to an extent not much greater than the total light R. At either edge of the strip, the brightness will be one-half that of the surface on each side, plus the light dispersed across the surface. The darkening would be distributed over a belt several times that of the actual belt, no definite limit being assignable, since it would fade off indefinitely in each direction. All that we can say with precision is that the total amount of darkening or abstraction of light would be equivalent to that produced by the central black band.

It is, of course, open to anyone to define the illumination, or the

amount of darkening, by algebraic formulae. But such formulae would have to contain as an unknown quantity the aperture of the objective, and to be complete should also include the defects of diffraction and atmospheric diffusion. The uncertainties thus arising are such that I conceive no practical gain would accrue by aiming at algebraic precision in the expression of the illumination in detail.

B. PSYCHOLOGICAL PRINCIPLES

Notwithstanding the volume of investigation on the psychology of vision, that branch of the subject which relates to accuracy of conception and estimate is, so far as the writer is aware, an almost virgin field. Two distinct processes are involved in vision. One is the conscious stimulus of the optical nerves by light, the other the perception by the mind of a real or supposed object indicated by such stimulus. The most remarkable property of vision is that it does not consist merely in taking account of the sensation, but is occupied almost entirely with the perceptive act, in which the sensation is dropped out of consciousness. Crude indeed would be a form of vision which ignored all but sensation. I shall use the term *visual inference* to describe the act by which the mind unconsciously draws conclusions as to an observed object from the image formed by its light on the retina. A fundamental property of this form of inference is that it comprises not only *seeing*, in the ordinary sense, but a rational interpretation or conclusion based on previous experience of what is seen.

This general proposition will be made clear by an example. Suppose oneself looking at a white line on a black background. We know from our experience in looking at a gas-lamp at night, or at a bright star, or even from a consideration of the refraction of the lenses of the eye, that the light emanating from a bright point is spread over a surface of several minutes' radius, which commonly increases with the age of the individual. Regarding it as a circle it it has not in any case a definite size, the light shading off gradually from the center outward. It follows that, when we look at a bright line, the image formed on the retina, even in the best eyes, must be 2′ or 3′ in breadth, howsoever thin the actual line may be.

But as a matter of fact, experience leads us to perceive only what

we should call a line, though we know rationally that it must have a sensible thickness. In other words, by the process of visual inference the eye does not perceive the line as it is actually pictured on the retina, but unconsciously introduces a correction based on general experience as to the geometrical form of the object which produces the image. It not only corrects the defects of the eye, but, by long experience, does this so completely that the defects pass in a large measure unperceived.

In this process we have a possible fruitful source of error of vision which, instead of being corrected by experience, tends to be strengthened by it. A mind accustomed to dealing with objects the correct perception of which depends mainly on visual inference, is naturally prone to extend that inference to cases where the conclusion would be illusory. Having this in mind, we see that observers trained in different ways may depict the same object very differently.

The process in question naturally plays a more important part as the object observed approaches the limit of visibility. If we can barely see an object in the darkness, we cannot distinguish its exact outline or character. Visual inference here comes in and assists the judgment as to the nature and character of the object. This may be the case even when the illumination is sufficient to render the object plainly visible, if only the mind is in a restful state. The fantastic character of the forms which may be seen in the coals of a fire by one sitting before it is well known. This example is not, however, pertinent to our present theme and is mentioned only as an additional illustration of this form of inference. As it is not possible within the limits of the present paper to discuss the subject in its generality, I shall limit myself to the special cases of lines approaching the limit of visibility.

As bearing on the question, I have made a number of experiments on the visibility and visual interpretation of dark lines on a white background. They differ from the similar ones made by Lowell in that, instead of taking the sky as a background and a distant wire as the dark line, I have used lines drawn with ink on paper, the latter being placed in a window and observed by transmitted light. This system was adopted, not only for convenience, but because the conditions in this way approach more nearly to those of actual observa-

tion on the disk of a planet, where the apparent background is not formed by the uniform blue light of the sky, but by a more or less mottled surface, and the lines are affected by atmospheric dispersion and telescopic aberration. Another point of difference was that, instead of making degrees of visibility, especially the *minimum visibile*, the main object, I sought to investigate the nature and limits of visual inference.

It is unnecessary to describe the experiments in detail, because they can be repeated in unending variety and by improved methods with great ease by anyone who desires to do so. The only particulars necessary are these. The lines were ruled lines about 0.7 mm in thickness, the length of all the lines being about 30 cm. One was continuous, others were broken at regular intervals by spaces 1 cm in length. There were also short lines from 1 cm in length upward. The lines being observed by transmitted light were not black, but gray and diffuse, thus corresponding more nearly to the telescopic vision than would black lines.

At a distance of 10 meters all the lines looked continuous and uniform. As the distance was diminished, the perception of the gaps did not come on suddenly, but by gradual steps. The first impression was that the lines were affected by irregularities in the form of thicker portions or blotches. The discontinuities were perceived gradually as the distance was diminished. The question thus suggested may be approached in this way. Consider or draw a line of considerable length so fine as to approach the limit of visibility. A segment L of this line can be taken so short as to be invisible. To avoid the question of an absolute *minimum visibile*, we may, if we choose, take a length L so short as to be perceived only with a minute intensity I. Now take from the whole line a segment of the length L; with what intensity will the absence of this segment be perceived? If we regard this impression as negative, we may say that the absence of this segment will be unperceived and that the mind will continue to perceive the negative impression. It follows that discontinuous portions of a line may be integrated into a continuous line.

Carefully testing this principle, the result might be stated in this way. The discontinuities were seen as such only at the distance at which the length L became certainly visible. At distances a little

greater than this there was a certain indefiniteness which made it impossible to decide whether a discontinuous line or one of varying thickness in different parts was being observed. The distance had to be increased only by a moderate fraction of its whole amount to render the perception of the line absolutely continuous. It seems to me that this principle affords as precise a statement as can be made of the conditions under which the process of visual integration, or perception of a discontinuous collection of objects as continuous, will take place.

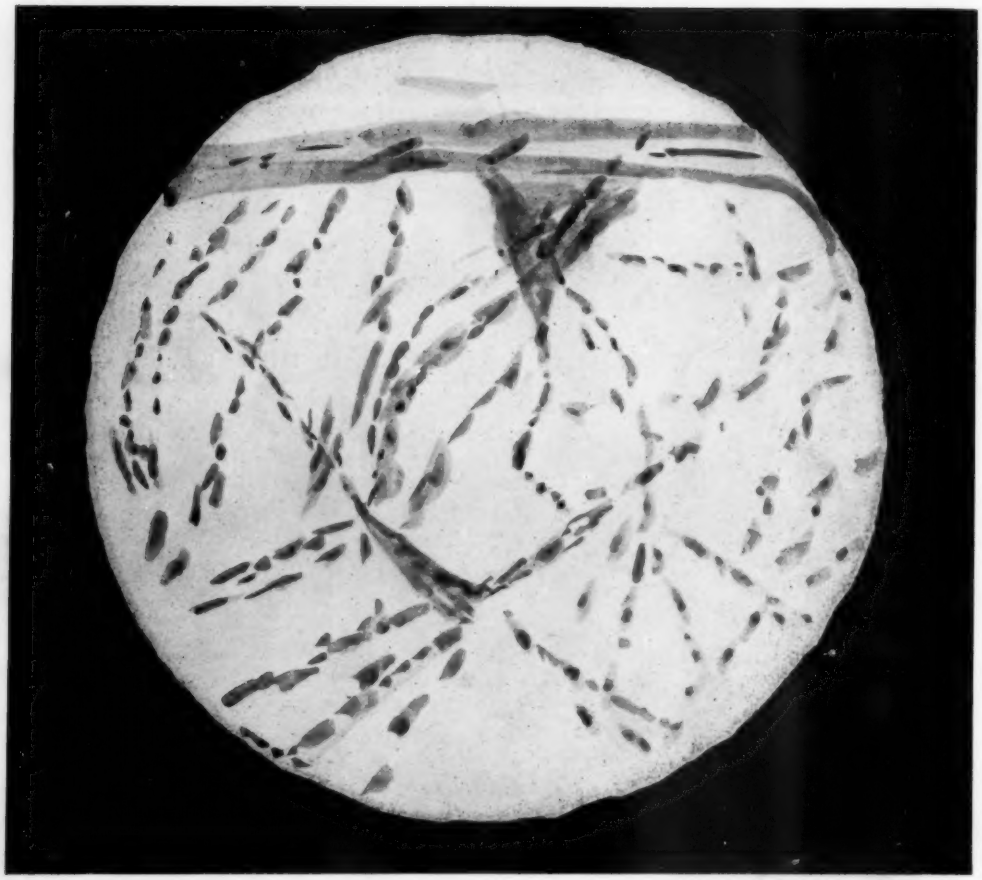
From what has been said we should regard the process of visual inference in this case as quite legitimate. Any error of judgment into which it leads us admits of rational correction, and such correction should be applied just as in the case of optical illusions or other sources of error. But the most surprising result of the few experiments I made was that the process assumed a form which, believing my visual habits to be at least as free from error as those of the ordinarily well-trained observer, was entirely unanticipated. When looking at the lines without exactly knowing where and what they were, I found that in one case what was faintly judged to be a continuous line up and down the paper was really a short line with a faint shade below it, which by visual inference was merged into the line, and led to the acceptance of its continuity across the paper. But a greater surprise was felt when a paper which I knew to have no visible lines upon it was in the window, and I fancied that I saw a system of continuous lines similar to that which I had been observing. So strong was this impression that, had I not known that the phenomenon was an illusion, I might have described or delineated the lines without any suspicion of their unreality. Going forward to see what caused the illusion, I found it due to an irregular shading of the tissue of the paper as seen by the transmitted light. The minutely darker ill-defined regions which were scattered all over the paper were integrated into lines like those I had been observing. I have understood that something of this sort has been noticed by others, but I was disposed to look upon the matter lightly until I experienced the illusions myself. Mr. Maunder's elaborate experiments in this direction, as described in the *Monthly Notices, R. A. S.* (**63**, 488, 1903), which were made upon schoolboys, have seemed to me open to question for two reasons. In the first place, experienced observers would

have been preferable to schoolboys; in the next place, drawings with a pencil were trusted, without (it would seem) there being any test by description, thus leaving a doubt, when a line was drawn with the pencil, whether it was anything more than an effort to represent a more or less indefinite shade by the aid of the pencil. It seems to me that we have here a very interesting field in which the best astronomical observers might well experiment upon each other by placing in a window a number of such papers, some with barely visible lines and others without, and determining the degree of certainty with which the two could be differentiated at various distances.

Through the courtesy of Professors E. C. and W. H. Pickering, and S. I. Bailey, the author is enabled to present the result of an experiment of this kind, which seems not devoid of interest. Besides lines drawn upon paper, observations on which seemed to confirm the general principle already suggested, a circular disk was prepared with light shadings, bearing a faint resemblance to what might be supposed to exist on *Mars*, but which did not contain any canal system of the kind commonly seen. This disk is reproduced on a reduced scale with sufficient precision in Plate I. Its breadth and distance were arranged so as to correspond to the apparent disk of *Mars* under the usual magnifying power. The markings were then drawn by Professors W. H. Pickering and Bailey, with the results shown in Plate II. Neither was made acquainted with the actual nature of the figures until after the latter had been made. It seems unnecessary to enter into any details of the conclusions to be drawn from a comparison of the original with the reproduction, beyond the remark that they seem to be affected by practice. Professor Pickering was an experienced observer of the canals of *Mars*, while Professor Bailey, though occasionally looking at *Mars*, had not made so long a study of the planet.

Additional sketches of the wash-drawing were subsequently made by Professor E. E. Barnard and Mr. Philip Fox of the Yerkes Observatory, from a distance of 96 feet. They had not seen the original from a less distance at the time their sketches were made. With their permission these sketches, which were made very carefully, and which resemble each other very closely, are also reproduced here. (Plate III.)

PLATE I



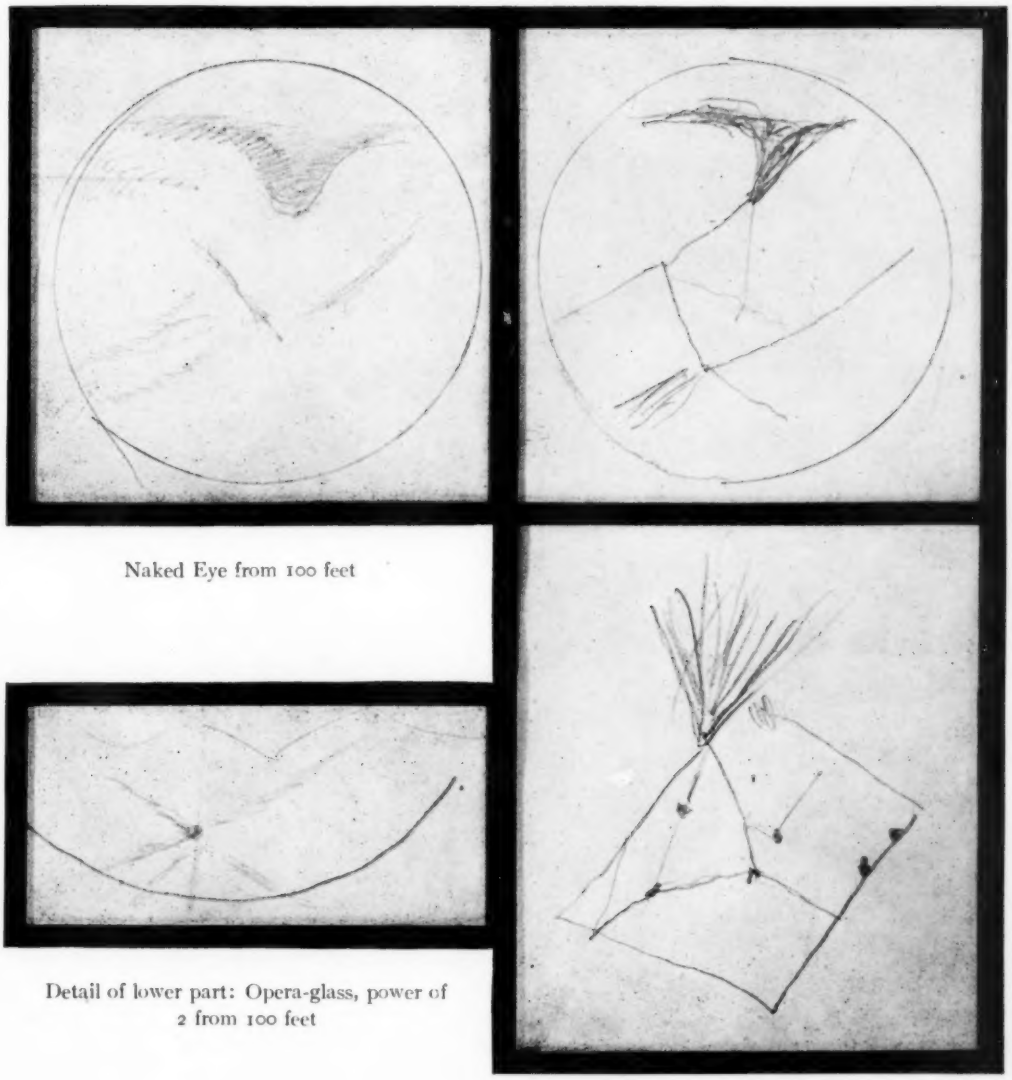
Wash Drawing by Author
Reduced from a diameter of 38 cm

PLATE II

SKETCHES

By S. I. Bailey

By W. H Pickering



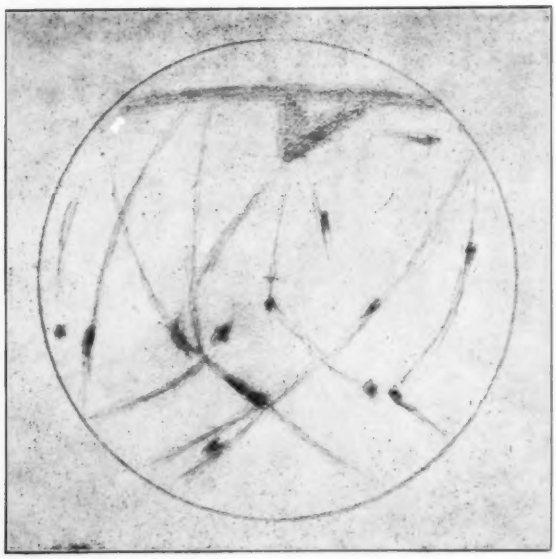
Naked Eye from 100 feet

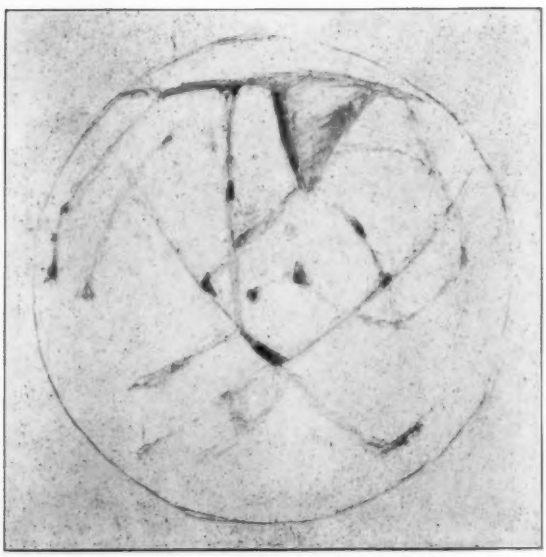
Detail of lower part: Opera-glass, power of 2 from 100 feet

The upper Sketch with Naked Eye from 30 meters. The lower, detail with opera-glass from 100 feet

PLATE III

SKETCHES
By E. E. Barnard
With Naked Eye from 96 feet





By Philip Fox
With Naked Eye from 96 feet

C. POSSIBLE INTERPRETATION OF THE CANALS OF *MARS*

Our next step is to apply the preceding result to the interpretation of the markings on *Mars*, specially of the canal system. Here at the outset we must guard against the error of considering this whole system as something which must stand or fall as a unit. Some of the markings which it has become the custom to designate by the term "canal" are seen in substantially the same position by so many observers that no question can be raised as to their subjective reality. In fact, several of them have been photographed. It is also to be considered that, as a general rule, the more practiced the observers, the more of these objects they see. But even admitting the subjective reality of them all, the considerations already adduced show that there is still room for doubt as to their interpretation, when we include the whole system of about 400 lines definitely catalogued by Lowell and his observers. It is one thing to say that the whole system of fainter canals is an illusion, and quite another thing to say that its objective reality may be very different from the subjective appearance.

The present discussion of the subject will be based almost entirely on the work of the Lowell Observatory. The excellence of the instrument and of the atmospheric conditions do not form the only justification for this selection. The unequaled continuity of the observations, the care with which the minutest details were looked for, and the generally critical character of their entire discussion, add to the force given by the favorable conditions.

We first remark that, at the distance of *Mars* at various oppositions, one second of arc will commonly be subtended by a linear distance on the face of the planet varying from 200 to 300 miles (320 to 480 km). If the opposition occurred absolutely at the perihelion, the equivalent might be as small as 175 miles (282 km). But the occasions on which it will fall below 200 are rare, while approximation to 300 must be the general rule, owing to the brevity of the period near an opposition when the distance will approach its minimum. The figure 200 miles or 320 km will therefore correspond to the most favorable ordinary case; and we shall for simplicity take it as the basis of computation.

Now, conceive an absolutely black line on *Mars*, of indefinite

length, and of a breadth of 3 or 4 miles (5 or 6 km). The angular geometric breadth of the strip may be 0″.01 to 0″.02. From what has been shown, and is elucidated in Fig. 3, it will be seen that the image of this line in the best terrestrial refracting telescope will not be black in any part, but will be spread out into a band of which the darkest part will be 0″.2 broad, bordered by a yet wider shade. It follows that even on the central line of the band, the actual amount of the darkening will be that produced by taking away about one-tenth or less of the light of the surrounding brighter regions of the disk. In other words, instead of a black line 0″.02 broad, we have a faint shade from 10 to 20 times as broad. In linear measure on the planet the apparent band will be 40 miles in breadth and upward, instead of 3 or 4. This spread of the darkness will necessarily diminish its visibility.

The question of the visibility of such a band is a difficult one, because no tests of visibility derived from comparatively unpracticed eyes would apply to the experienced observer. It would, in fact, be almost impossible to estimate the *minimum visibile*, but for the observations bearing on the case by Lowell himself. He found that a wire projected on the sky vanished from sight with increasing distance only when its breadth subtended a diameter less than 0″.69. Another datum is offered by what I believe is an established fact of photometry, that upon a uniformly illuminated surface of the best brightness a change of 1 per cent. of the light is perceptible.

The data, however, cannot lead us to a definite conclusion, because, in order that a shade may be visible, it must have a certain breadth. From observation and reasoning which I need not detail I should regard several minutes as the minimum breadth necessary for a 1 per cent. shade to be visible. It goes without saying that the breadth of 0″.69 is visible only when black and when seen with perfect sharpness. If the blackness is spread out into a band by aberration, how much must the breadth of the wire be increased for a given breadth of band? It may be remarked that, if the breadth is multiplied 100-fold, we shall have a limit of visibility in a broad band indicated by the second principle just cited. But the breadth of the band would then be only 69″, while from the rude observations I have made it may be inferred that the breadth of the shade when thus spread out would have to be

multiplied four or five times before becoming visible to an eye which could see it when sharply defined.

This statement presupposes that the bright background is perfectly uniform. This is not the case with *Mars*, and there is no doubt that the amount of the blackness for the ***minimum visibile*** would have to be multiplied several times on account of the variety of shading in the visible disk of the planet. To spread a band of 0".2 in breadth to 5′ would require a magnification of 1500. I doubt if anything like this power could be advantageously applied. With a power of 500 the apparent breadth would be 1′ 40".

My observations lead to the conclusion that on a quite uniform background this breadth of darkening will be visible, but that, if the background is in any way mottled, the mottling will be combined with the band in such a way that the judgment of the breadth may be quite illusory. If two objects just below the ***minimum visibile*** are combined, the combination may be visible as a single object; but the subjective effect may be very different from the objective reality. Only the roughest estimates are possible in the case; and I conceive that, subject to this drawback, although a perfectly black line 3 miles (5 km) in breadth might be visible on *Mars* if the surface of the planet were perfectly uniform, the breadth would probably have to be increased to 8 or 10 miles (13 or 16 km) clearly to differentiate the line from the markings in its neighborhood.

But it cannot be supposed that the canal system is absolutely black in color. Whatever its nature, we must suppose that its albedo is half or more of that of the surrounding regions of the planet. We must therefore, on this hypothesis, double the actual breadth. My conclusion is that the actual breadth of the narrowest visible canals on *Mars* must exceed 10 miles (16 km), and may be as great as 20 miles (32 km). Adding the border of 20 miles on each side necessarily produced by aberration, diffraction, and softening, the apparent breadth in the telescope and on the retina would be 50 miles (80 km) and upward.

This result differs widely from that of Mr. Lowell, who estimates 2 or 3 miles as the breadth of the narrower canals.[1] The source of the deviation of my result from his is quite simple. He compares the

[1] *Mars and Its Canals*, p. 182.

canal with a wire seen against a sky and therefore perfectly black. He also implies the image of this black line, about 0″.01 in minimum thickness, to be perfectly sharp on a uniform background. Increase its apparent breadth from this limit up to the diameter of the aberration circle in the best telescope, and suppose a half-blackness, and we shall have to multiply this breadth by perhaps 5 or 10 to correspond to the different hypothetical conditions. It is not necessary, however, to insist on the actual breadth of the narrowest line, because, whatever it may be, it will on the retina be increased by aberration, no matter how narrow the objective reality.

Let us now consider the entire system of 398 canals named and catalogued in the *Annals* of the Lowell Observatory.[1] It seems that 2000 miles is the common length of a canal, while many exceed 2500. Assuming 400 canals of a mean length of 1500 miles (2400 km), let us compute the area which the entire system will subtend on the retina of the terrestrial eye with the aid of the best refracting telescope.

The area of a mean canal in square miles will be

$$400 \times 1500 \times \text{Breadth} = 600{,}000 \times \text{Breadth}.$$

Then, taking in succession (1) a mean actual breadth on the planet of 7 miles as assigned by Lowell; (2) a mean of 15 miles, which, if my reasoning is correct, must be nearer the truth, in view of the canals not being black; (3) an enlargement of 40 miles by aberration, etc.—we shall have the results:

(1) Canals black: objective area, 4,200,000 square miles.
(2) Canals half-tone: objective area, 9,000,000 square miles.
(3) Canals enlarged: apparent area, 33,000,000, square miles.

The actual surface of *Mars* is about 55,000,000 square miles. We conclude:

Making due allowance for the aberration of the best achromatic telescope, the total area of the entire system of 400 canals, as depicted on the retina of the terrestrial eye, can scarcely fall much below one-half the total area of the planet, and may be greater. In fact, were all the canals on the disk visible simultaneously, it would be difficult to establish their reality, because several canals in the same neighborhood would interfere with each other's visibility. But it is understood

[1] Vol. III, pp. 268–277.

that such is not the case. Many of the fainter canals are variable and visible only occasionally. The preceding computations therefore give rather the total part of the surface that may be covered by the canal system than the actual area of the system as seen at any one time. Although these results may weaken the probability of the reality of the entire canal system, it does not disprove its possibility. In fact, it is quite consistent with Lowell's fundamental explanation of the phenomena. At the same time, it shows how wide is the possible field of interpretation, and explains the difficulty which many observers have encountered in tracing the canals. So complex a network within a disk only 20″ in diameter could not but be interpreted largely according to the experience and habits of the observer.

Although in this discussion the writer has not questioned the subjective reality of the canal system, he cannot but feel that the proof of its objective reality is incomplete until the observers of the system investigate the processes of visual inference in their own eyes. This involves no serious difficulty, being little more than an extension of the rude experiments described in the present paper. The experiments should be made by an independent agent preparing drawings, representing on sheets of white paper forms similar to those which might be supposed to prevail on the surface of *Mars*, in as great a variety as possible. These forms should then be studied by the observers without an advance knowledge of the details of each, and conclusions as to the nature of each drawing, and its resemblance to the Martian canal system, should be recorded by drawings and description. Then leaving a priori probabilities aside, the a posteriori probability would be in favor of the drawings which, in the opinion of the observer, most nearly resemble what he has been accustomed to see on *Mars*.

WASHINGTON
May, 1907

ARC SPECTRA UNDER HEAVY PRESSURE

By W. J. HUMPHREYS

The effects, discovered several years ago, of pressure of 10 to 12 atmospheres on arc spectra[1] indicated that results of some value might be found by examining spectra produced under much greater pressures. At that time higher pressures could not be obtained, but during the spring of 1906 I secured, with specially constructed apparatus, nearly two hundred spectrogram negatives, each 20 inches (50 cm) long, taken at pressures of 42, 69, and 101 atmospheres; to these some twenty supplementary ones have recently been added.

More than one hundred of these spectrograms have been selected and studied, only those being used that were taken when all parts of the apparatus were in careful adjustment, that had lines satisfactorily measurable, and that were above suspicion of accidental displacements during exposure.

A Rowland concave grating of 14,438 lines per inch (568 per mm) and 21.5 feet (6.5 m) focal length was used, and nearly all the negatives were taken in the second- and third-order spectra. The pressures were obtained by forcing air with a four-stage Norwalk compressor into a forged-steel bottle that contained the arc, and read by Crosby gauges, afterward tested by the Bureau of Standards and found to be substantially correct.

During each exposure the pressure was maintained practically constant by having the space about the arc in open communication, through a steel tube, with the compressor which was kept running with its blow-off valve properly set. The pump, which ran very smoothly, and the spectroscope were both located on basement floors, but in different rooms, and so far separated, about 50 feet (15 m), that no disturbance of the spectrum lines could be detected, though it was carefully searched for both visually and photographically. This freedom from disturbance probably was due in part to the fact that neither instrument was mounted on ground piers, and that there was nowhere any solid connection between them.

[1] Humphreys and Mohler, *Astrophysical Journal* **3**, 114, 1896; Humphreys, *ibid.*, **4**, 249, 1896 and **6**, 169, 1897.

The arc was produced by a 220-volt direct current of usually about 15 amperes. With this particular voltage, the highest easily available, it was not possible to maintain a satisfactory arc between fixed electrodes at the pressures used, and therefore one of the poles was rotated. This part of the apparatus is described on pages 36 to 40 in this number. The rotating pole was always the anode, and in nearly all cases consisted of carbon. The fixed pole was either carbon or a metal rod, depending on the element under examination. Commercial arc-light carbons, with iron, titanium, calcium, aluminium, and a few other impurities, gave good results, as did also such carbons after being soaked in salts of certain elements. Excellent results were likewise gotten with metal rods or blocks of iron, chrome iron, self-hardening steel (iron, chromium, manganese), brass, copper, aluminium, and nickel.

The photographic plates employed were the Seed "Gilt Edge," and the Cramer "Isochromatic Instantaneous," the former for much the greater part of the work—all except that done in the green, where the latter kind is more sensitive.

The negatives were obtained in the well-known way of exposing a narrow strip along the middle of the plate, with the sides protected, to the arc under pressure; and then with this part protected exposing the sides of the plate to the arc at atmospheric pressure.

The method of measurement and reduction was the same as that described in a former paper,[1] by which each line was twice measured and possible bias in setting practically eliminated. Besides, many of the lines appear on a number of different plates, and they were carefully measured—violet to red and then red to violet—on each plate and the general average taken of all measurements of any given line at each pressure.

The measuring-machine used was furnished by Gaertner & Company, of Chicago. The screw has a millimeter pitch and reads directly to thousandths of a millimeter. The accuracy of the screw was not directly tested, but in identifying lines the wave-lengths computed from measurements agreed very closely with those given in the best tables, and I am confident therefore that such irregularities of the screw as may exist are well within the errors of measure-

[1] *Astrophysical Journal*, **6**, 180, 1897.

ment, except possibly in the case of very narrow or finely reversed lines—such as one never gets under heavy pressure.

The adjustments of a concave grating spectrograph when mounted on tracks at right angles to each other, as used by Rowland and as adopted in the present case, are so well known that ordinarily no description is called for; but in studying displacements or shifts of spectrum lines particular attention must be given to placing the slit rigidly parallel to the rulings on the grating. When this adjustment is not accurately made, a difference in position of the image of the arc on the slit, which often occurs, will produce a displacement of the lines that may be most misleading.

If one has command of sunlight, it is easy properly to set the slit by observing a sharp line and having an assistant operate the slit till the line becomes narrowest and best defined. Arc spectra, too, may similarly be used, though ordinarily not so satisfactorily, owing to the relatively feeble light in the case of very narrow lines. It is also possible to take a series of negatives with the slit in different but known positions, and from these to determine its proper position. Again, by eclipsing the middle part of the slit, taking the light through only its ends, and adjusting it till the two lines thus produced become continuous with each other, an excellent setting may be secured. Finally, if the tracks along which the grating and camera move are horizontal, one can use a silk or other fiber with a weight attached to it — a plumb-line — very close to the end of the ruling on the grating, and adjust the grating until its ruling is quite accurately vertical. The plumb-line can then be placed close to the slit and the latter easily brought also to a vertical position, and therefore parallel to the rulings on the grating.

In my own work, not having access to sunlight, I finally adopted this latter method, but checked it up with all the others.

This particular adjustment is described in detail because in studying changes in wave-length it is vital, so much so that an approximation to parallelism quite sufficient for many purposes may in this case lead to serious errors.

One of the most important effects of pressure on a spectrum line is the shifting of the maximum of its intensity toward the red, an increase of its wave-length, or an increase in the period of vibration

TABLE I

PRESSURE-SHIFT

ALUMINIUM

λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.	λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.
3082.27	0.093			3944.16	0.190	0.314	0.372
3092.95	.087			3961.68	.180	.310	.387

BARIUM

3071.71	0.070			4934.24	0.120		
4554.21	.095	0.162	0.210				

CALCIUM

3179.45			0.248	4226.91[3]	0.159	0.265	0.398
3933.83[1]	0.065	0.104	.154	4302.68	.085		
3968.63[2]	.080	.136	.203	4318.80	.072		

[1] K, [2] H, [3] *g*.

CHROMIUM

2835.75	0.051			4026.30	0.080		
3120.51	.036			4027.24	.076		
3433.72	.074			4039.21	.067		
3436.31	.058			4048.94	.070		
3578.81	.074	0.142		4058.89	.076		
3593.57	.095	.120		4065.84	.120		
3605.46	.083	.150		4067.05	.044		
3615.76	.050			4077.81	.069		
3632.92	.080			4126.67	.040		
3646.26	.045			4171.81	.050		
3666.10	.054			4179.37	.080		
3885.35	.060			4190.32	.058		
3894.20	.072			4191.41	.040		
3908.87	.049			4193.80	.101		
3916.38	.062			4195.09	.083		
3919.31	.052			4198.65	.090		
3921.20	.076			4203.71	.031		
3926.80	.166			4204.61	.087		
3928.79	.050			4208.50	.090		
3941.66	.046			4209.50	.091		
3963.82	.080			4209.90	.048		
3969.89	.063			4221.71	.080		
3976.81	.058		0.160	4240.82	.045		
3984.02	.140			4254.49	.056	0.075	0.130
3990.14	.056			4263.28	.064		
3991.26	.070	.125		4274.91	.076	.123	
3992.95	.066			4280.53	.061		
4001.58	.084			4289.87	.087	.130	.218
4012.63	.080			4295.92	.056		
4022.38	.066			4297.91	.061		
4023.90	.065			4301.33	.052		
4025.14	.061			4323.70	.050		

TABLE I—*Continued*

CHROMIUM—*Continued*

λ	At 42 Atmos. Å. U.	At 60 Atmos. Å. U.	At 101 Atmos. Å. U.	λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.
4344.66	0.057	0.085	0.128	4646.33	0.065	0.100	
4351.91	.065	.075		4651.44	.095		
4359.78	.066	.110		4652.31	.058	.088	
4363.25	.064			4680.65	.056		
4371.44	.060	.084		4729.89	.129		
4497.02	.040	.069		4730.88	.101		
4526.65	.080			4756.30	.146		
4535.95	.075			4792.61	.183		
4546.15	.060			5204.67	.164		
4580.22	.040			5206.20	.156		
4600.92	.085			5208.58	.092		
4613.54	.050			5247.68	.132		
4616.28	.053	.089		5348.50	.196		
4626.31	.056						

COBALT

3894.21		0.143		4092.55	0.042		
3995.45	0.072	.131	0.150	4118.92		0.094	
3998.04	.054			4121.47	.070	.104	

COPPER

2883.03		0.040		3274.06	0.090	0.230	
2961.25		.052		4242.42		.473	
2997.46		.046		4249.21		.273	
3010.92		.044		4259.63		.387	
3063.50		.041		4378.40		.420	
3073.89		.026		4415.79		.409	
3094.07		.032		4539.98		.459	
3194.17		.047		4587.19		.513	
3247.65	0.090	.145		4704.77		.460	

IRON

2931.55	0.026			3307.33	0.030		
2991.78		0.062		3314.86	.120		
3037.54	.052			3323.84	.073		
3047.71	.053			3329.00	.052		
3050.90		.049		3355.27	.064		
3059.19	.063			3366.88	.035		
3067.30		.071		3369.62	.048	0.110	
3175.53	.068			3370.87	.050		
3229.19	.013			3380.17	.054		
3233.14	.038			3384.05	.030	0.50	
3254.47	.060			3392.37	.036		
3265.73	.091			3392.74	.069	.120	
3271.12	.100	.123		3394.65	.040	.070	
3291.10	.168			3399.39	.045	.074	
3298.25	.050			3401.60	.080		
3306.50	.072			3402.33	.060		

TABLE I—*Continued*

IRON—*Continued*

λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.	λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.
3404.41	0.055	0.076		3647.99	0.090	0.135	
3407.55	.060	.108		3649.65	.060		
3411.43	.035			3650.42	.025		
3413.22	.056	.088		3651.61	.065	.105	
3414.83	.060	.068		3659.65	.050	.080	
3415.61	.060			3669.65	.050	.060	
3417.92	.058	.097		3670.20	.047		
3418.58	.060	.100		3676.44	.050	.086	
3422.69	.056			3677.76	.052		
3424.36	.052	.090		3680.03	.062	.067	
3425.08	.080			3683.18	.040		
3427.21	.053	..104		3684.24	.053		
3428.26	.060			3687.58	.090	.120	
3440.69	.050			3689.58	.084		
3441.07	.050			3695.18	.070		
3443.96	.045	.066		3703.68	.086		
3445.22	.054			3704.59	.046		
3447.37	.050			3705.70	.054	.070	
3450.41	.057			3709.37	.095	.140	
3451.99	.059			3716.04	.107		
3458.39	.111			3720.07	.047	.070	0.091
3465.95	.050	.067		3722.69	.050	.084	
3471.40	.040			3724.51	.054		
3475.52	.048	.066		3727.78	.100	.138	
3476.75	.036	.059		3733.46	.050	.080	
3485.42	.065			3735.00	.092	.150	.180
3490.65	.052	.080		3737.27	.040	.065	.093
3495.37	.048			3738.44	.078		
3497.20	.050			3743.58		.155	
3497.92	.045	.075		3745.67	.050	.086	
3506.59	.047			3745.95	.050		
3508.58	.095			3748.39	.040	.063	.090
3513.91	.063	.100		3749.61	.085	.160	.180
3521.36	.085			3752.57	.098		
3558.62	.085	.150		3758.36	.090	.140	.184
3565.50	.074	.128		3763.90	.095	.180	.195
3570.23	.075	.141		3765.66	.106		.160
3581.32	.083	.133		3767.31	.118	.160	
3585.43	.100	.162		3788.01	.090	.180	
3585.84	.076			3795.13	.093	.135	
3587.10	.072	.127		3798.65	.085	.170	
3603.34	.062			3799.68	.075	.140	
3605.62	.030			3805.47	.092		
3606.83	.059			3813.12	.058	.101	
3608.99	.072	.117		3815.97	.110	.175	.180
3618.92	.080	.120		3820.56	.125	.172	.200
3621.61	.068			3824.58	.040	.050	.070
3622.15	.060			3826.04	.090	.140	.200
3623.33	.040			3827.96	.102	.150	
3631.62	.090	.143		3834.37	.110	.165	.210
3638.44	.061			3840.58	.098	.165	.237
3640.53	.065			3841.19	.100	.180	

TABLE I—*Continued*

IRON—*Continued*

λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.	λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.
3850.11	0.082	0.173	0.235	4236.09	0.274	0.435	
3856.49	.038	.057	.070	4245.39	.060		
3860.03	.042	.071	.105	4250.93	.089	.138	0.180
3865.65	.103	.180		4260.64	.246	.399	.540
3868.03			.082	4271.93	.083	.132	.185
3872.61	.108	.151		4282.58	.043	.061	.093
3878.82		.066		4294.26	.084	.130	
3886.38	.056	.083	.101	4307.96	.090	.140	.214
3887.17	.073	.130		4315.21	.036	.051	.097
3888.63	.089	.174		4325.92	.097	.143	.189
3893.47	.072			4337.14	.090	.152	
3895.75	.030	.047		4352.86	.052	.074	
3899.80	.036	.067	.070	4367.68	.060	.100	
3903.06	.095	.151	.170	4369.89	.055	.090	
3904.00	.056			4376.04	.039	.060	
3906.58	.050			4379.36	.101		
3920.36	.033	.060	.077	4383.70	.125	.153	.180
3923.00	.032	.060	.070	4384.82	.130		
3928.05	.038	.064	.080	4404.88	.110	.150	.207
3930.37	.047	.065	.090	4407.80	.180		
3948.87	.050		.115	4408.54	.160		
3950.05	.066		.130	4415.27	.087	.146	.220
3956.77	.036			4422.67	.065	.105	
3969.34	.089	.148		4427.44	.055	.086	
3977.83	.042	.077	.105	4430.74	.190		
3981.87			.150	4442.46	.190		
3984.08	.085			4443.30	.060		
3986.27	.061			4447.85	.180		
3997.49	.048	.076	.087	4454.50	.080		
3998.16	.066			4459.24	.160	.203	.250
4005.33	.103	.152	.215	4461.75	.060		
4009.80	.040	.075		4466.70	.056	.074	
4014.63	.050	.082		4476.20	.072	.120	
4017.23	.062			4482.35	.125		
4021.96	.037	.073	.108	4494.67	.200	.290	
4045.90	.103	.170	.200	4528.78	.172	.250	
4063.63	.107	.168	.201	4531.25	.075		
4071.79	.092	.178	.260	4547.95	.097	.180	
4107.58	.060	.109	.145	4592.75	.110		
4109.88	.062	.092	.115	4603.03	.093	.150	
4118.62	.085	.110	.190	4647.54	.070		
4132.15	.105	.212		4662.09	.067		
4134.77			.138	4691.52	.070		
4143.96	.116	.198		4710.37	.060		
4156.88		.096	.148	4736.91	.085		
4172.81	.140			4762.48	.160		
4181.85			.170	4786.91	.076		
4184.99	.040	.060	.085	4789.74	.080		
4199.19	.073	.120	.190	4859.86	.390		
4202.15	.071	.130	.173	4871.43	.420		
4219.47	.074	.111	.145	4878.33	.400		
4233.76	.240			4919.11	.375		

TABLE I—*Continued*

IRON—*Continued*

λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.	λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.
5171.71	0.075			5429.74	0.085		
5195.03	.080			5434.66	.120		
5269.65	.083			5447.05	.095		
5328.15	.100			5455.80	.105		
5371.62	.095			5497.52	.110		
5397.27	.080			5501.61	.095		
5400.60	.063			5506.92	.120		
5405.91	.100			5615.81	.080		
			LANTHANUM				
3916.16	0.030			4086.86	0.144		
3929.34	.092			4238.55	.098		
3988.69	.076			4269.64	.085		
3995.90	.118			4280.43	.058		
4031.85	.088			4430.09	.051		
4037.26	.064			4570.20	.066		
4043.04	.170						
			LEAD				
3639.71			0.306				
			MAGNESIUM				
2852.22		0.160	0.190	5183.84	0.275		
			MANGANESE				
3200.06	0.070			4063.38	0.106		
4018.25	.070	0.120		4083.75	.080		
4030.87	.052	.090	0.150	4266.08		0.084	
4033.18	.050	.091	.112	4451.75	.180		
4034.60	.050	.091		4762.54	.145		
4035.88	.086			4766.58	.158		
4041.49	.068	.103		4783.60	.290		
			NICKEL				
3002.60			0.107	3369.66	0.077		
3003.73			.103	3372.12	.048		
3012.10			.105	3374.35	.029		
3038.05			.097	3380.70	.096		
3050.88	0.032	0.077	.101	3391.21	.070		
3054.40	.041		.102	3393.10	.063		
3057.72	.050	.090	.127	3414.90	.077		
3101.61	.048			3423.80	.084		
3102.00	.059			3433.71	.094		
3134.26	.060		.122	3437.45	.063		
3233.11	.049		.115	3446.34	.071		

TABLE I—*Continued*

NICKEL—*Continued*

λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.	λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.
3452.98	0.062			3664.24		0.110	
3458.51	.091			3670.57		.088	
3461.78	.067			3674.28		.070	
3467.63	.063			3688.58		.068	
3469.64	.095			3722.63		.111	
3472.68	.080			3736.94		.083	
3493.10	.081			3775.71		.088	
3501.00	.050			3783.67		.058	
3510.47	.083			3807.30		.076	
3519.90	.075			3858.40		.117	
3524.65	.096			3972.31		.075	
3548.34	.080			3973.70		.140	0.176
3561.91	.063			4331.78	0.088	.150	.204
3566.51	.091			4401.70			.480
3571.99	.100			4459.21			.625
3588.08	.092			4470.61		.580	
3597.84	.102			4520.20		.120	
3602.41	.082			4592.69	.320	.620	
3609.44	.072			4600.51	.464		
3610.60	.101			4605.15	.280	.600	
3612.86	.080			4648.82	.270	.660	
3619.52	.065			4686.39	.325	.557	
3624.87	.060			4714.59	.274		
3662.10	.053			4756.70	.297		

PALLADIUM

3002.74	0.052			3460.93	0.087		
3028.05	.056			3481.31	.096		
3065.41	.082			3517.08	.058		
3114.19	.066			3634.85	.063		
3142.97	.040			3690.49	.070		
3219.08	.050			3894.33	.098		
3251.89	.085			3958.79	.119		
3259.01	.094			4213.11	.130		
3287.38	.094			4388.80	.290		

POTASSIUM

4044.29	0.504			4047.36	0.480		

SILICON

2881.70	0.080			3905.70	0.184		

STRONTIUM

4077.88	0.070			4607.52	0.170	0.268	
4215.66	.100						

TABLE I—*Continued*

TITANIUM

λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.	λ	At 42 Atmos. Å. U.	At 69 Atmos. Å. U.	At 101 Atmos. Å. U.
3186.58	0.121			3989.92	0.049	0.097	0.140
3200.08	.057			3998.77	.047	.078	.130
3234.68	.042			4009.06	.055	.081	
3236.72	.033			4021.98	.038	.064	
3242.15	.035			4286.15	.103		
3253.04	.044			4287.55	.087		
3349.56	.027	0.046		4291.07	.115		
3354.80	.035	.054		4295.91	.100		
3361.41	.037	.077		4300.73	.104		
3370.61	.045			4301.23	.110		
3371.62	.030	.050		4306.07	.104	.150	
3372.91	.034	.067		4318.83	.042		
3373.03	.025			4427.28		.037	
3386.10	.051	.092		4533.42	.176	.270	
3387.97	.030			4534.97	.124	.195	
3461.69	.080			4544.83	.080		
3635.61	.058			4682.08	.077		
3642.82	.053			4691.50	.080		
3653.61	.050			4758.30	.067		
3685.30	.012			4759.44	.092		
3904.95	.073			4841.00	.029		
3921.56	.068			4981.92	.077	.152	
3948.80	.045			4991.24	.135	.212	
3956.45	.030			4999.67	.120	.165	
3958.33	.045			5007.42		.225	
3981.91	.056	.097	150	5013.45	.056	.100	

TUNGSTEN

3300.97	0.040			4083.13	0.096		
3311.53	.038			4102.85	.036	0.050	0.082
3326.33	.027			4241.62	.086		
3331.84	.047			4244.52	.060		
3361.26	.071			4269.53	.064		
3373.89	.046			4484.37	.030		
3429.72	.077			4610.12	.087		
4008.91		0.080		4660.00	.071		
4015.39	.060			5053.50	.044		
4019.37	.070	.128					

ZINC

3075.99		0.056		3740.12		0.250	0.320
3683.63		.220	0.350	4058.02	0.200	.280	.382

of the luminous particle. Table I shows the measured amount of this increase of wave-length in thousandths of an Ångström unit for each line measured, and for each pressure under which it was taken.

The wave-lengths used for aluminium, barium, calcium, copper, iron, lead, magnesium, potassium, strontium, and zinc are taken from tables given by Kayser and Runge; those for chromium, cobalt, manganese, nickel (in part), and titanium (in part), from Hasselberg's tables; and those for lanthanum, nickel (in part), palladium, silicon, titanium (in part), and tungsten, from tables by Exner and Haschek.

It seems convenient to list some of the more conspicuous effects of pressure on arc spectra, and, whenever necessary, to comment on each separately.

1. The brilliance of the arc becomes much greater as the pressure, if due to atmospheric air, with which all my work was done, is raised. What the effect of wholly inert gases would be, I cannot say. Presumably this is caused by the more rapid wasting-away of the electrodes; possibly because of the increased resistance and shortened arc—a greater concentration of energy and it may be a corresponding increase in temperature—though the effect of pressure on the temperature of the electric arc seems to be an open question.

2. Reversals are decidedly more pronounced and frequent under heavy than under light pressures, and especially so in the ultra-violet region. This probably is due to a denser layer of absorbing vapors surrounding the arc, and, like 1, may, in part at least, be accounted for by the more rapid burning of the electrodes.

3. Pressure seems to increase the width of all lines, though quite unequally, and to make them somewhat nebulous, especially at the edges. Occasionally lines are found on certain plates, that appear to be narrowed by heavy pressure; but probably this is due to a decrease in exposure, since under these conditions they are rather weak, and therefore only show at their places of maximum intensity.

4. The lines of the carbon (cyanogen) bands are not appreciably displaced, if at all, even at the highest pressures used; though, like the individual lines of the elements, they are increased in width.

5. The wave-lengths of all other lines examined increase approximately proportional to the increase in pressure up to the highest used, though this increase or shift is very different, not only for different elements, but even for different lines of the same element.

6. The amount of shift of a given line is practically independent

of whether or not it is reversed; that is, the emission and the absorption are similarly and equally affected.

7. In general the pressure-shift of the spectrum lines seems to increase with the wave-length, but probably this is true only of lines of the same series; at any rate, it is not conspicuous in the case of iron, nickel, and other elements whose lines appear to belong to many series, or to none.

8. So far as I can judge from the scanty numerical data on the Zeeman effect, in general those lines which are strongly separated by a magnetic field are correspondingly largely displaced by pressure; and conversely those, like the lines of bands, that have but little if any Zeeman separation, are but slightly if at all shifted by pressure.

9. The relative intensities of lines at high and low pressures vary exceedingly. In general, the intensity of titanium lines increases with increase of pressure, while iron in this particular behaves most irregularly; some of its lines become more intense as the pressure is increased, while others are diminished, a number entirely disappearing even at 40 atmospheres. Among those that disappear at this pressure are λ 4222.32, 4250.28, 4299.42, 4878.33, 5049.94, and 5191.56.

As already explained, the electrodes burn away more rapidly as the pressure is raised, and therefore the increased intensity of the lines might be expected from the greater rate of supply to the arc of the material to which they are due, and possibly, too, in part to the increased potential gradient. But the opposite effect, the enfeebling of many lines, calls for some other explanation. It may be that their emission, too, is increased, but more or less neutralized by a large absorption factor. Just why this should be so, if indeed it is the correct explanation, is not clear; but neither is it obvious why lines in the open arc, for instance, differ so greatly in the phenomena of reversal, that is, in their relative amounts of emission and absorption.

Since sun-spot lines give similar effects,[1] some of them being intensified, while others are enfeebled, I sought to determine whether there was any connection between sun-spot and pressure phenomena. Unfortunately, however, enfeebled sun-spot iron lines are so faint

[1] Hale and Adams, *Astrophysical Journal*, **23**, 11, 1906; Adams, *ibid.*, **24**, 69, 1906.

in the arc that they do not show appreciably on my plates. However, λ 5191.56, which vanishes under heavy pressure, is somewhat intensified in sun-spots. So far, then, as the evidence of a single line is worth anything, it would appear that the light in sun-spots does not come from any great depth in the sun's atmosphere—that the spots, whatever their cause, are distinctly surface phenomena. A careful examination of properly selected lines for pressure-shift in spots should be of value in this connection.

10. The shift of spectrum lines seems to be chiefly a function of total and not of partial pressure; that is, the displacement of a line does not greatly, if at all, depend upon the amount of material in the arc to which the line in question is due. Kayser, among others, has shown this to be true at atmospheric pressure, and, if true at one pressure, there is no very clear reason why it should not be equally true at others.

It has been claimed,[1] that the shift is a function of partial pressure, but my plates do not justify this conclusion. While it is true that the measurements of different plates do not always closely agree, nevertheless these differences are generally referable to some instrumental disturbance, all lines on the plate being similarly affected, or else apply to lines whose errors of measurement are very large. At any rate, they are such that I do not feel justified in referring them to anything other than mere accident.

11. The increase in wave-length of the spectrum lines is only a small part of that which would be expected if the luminous particles behaved like Hertzian oscillators.

The period of such an oscillator is known to be expressed by the equation

$$T = 2\pi\sqrt{LC},$$

in which T is the period, L the inductance, and C the capacity. With other things equal, C increases directly with the inductive capacity of the dielectric in which the oscillator is placed.

It is also known that in the case of gases $\mu_\lambda - 1 = ad$, where μ_λ is the refractive index for wave-length λ at the density d, and a is a constant; but for all ordinary pressures $\mu_\lambda - 1$ is small, and therefore so too is a.

[1] Huff, *Astrophysical Journal*, **14**, 41, 1901.

But since $\mu^2 = K$, the specific inductive capacity, we have $K = (ad+1)^2$ or $d(a^2d+2a)+1$.. Therefore $K-1 = 2ad$ very nearly; that is, K is approximately a linear function of the density. Now, since K for air at ordinary density is about 1.0006, and for air at a pressure of 100 atmospheres roughly 1.06; then, neglecting temperature effects, we might expect, on the assumption that spectrum lines are due to Hertzian oscillators, that the period at one atmosphere will be to that at one hundred atmospheres as $\sqrt{1.0006}$ is to $\sqrt{1.06}$; or, in symbols, that

$$\frac{\lambda_1}{\lambda_{100}} = \frac{\sqrt{1.0006}}{\sqrt{1.06}};$$

and therefore "g," of wave-length 4227 at 1 atmosphere, would become λ 4350.6 at 100, or be shifted 123.6 Ångström units. But its measured shift for the same conditions is only 0.4 of an Ångström unit, or one three-hundredth of the calculated amount. Besides, lines of the same wave-length should have equal shifts, which is not in accord with experiment. From these facts it seems unlikely that the increase in the specific inductive capacity of the region in which a substance is placed can be the cause of the pressure-shift of its spectrum lines; nor is it at all clear just how it could be, since the interior of any given atom probably is not altered by changes in the density of the surrounding gas.

This same line of argument has been offered by W. B. Anderson,[1] but I venture to discuss it rather fully because the statement in a former paper[2] that it would not be easy to decide the question experimentally has led to some misunderstanding.

The effects of different gases might be tried on arc spectra, as has been done in an excellent piece of work on spark spectra,[3] but this is already in great measure secured by using metal poles, and carbon poles with metallic impurities; and the difference in the shifts, as already explained, is not decisive. Besides, the difference in the inductive capacities of the gases is a less percentage of the total of either than the error of measurement is of the total shift;

[1] *Astrophysical Journal*, **24**, 253, 1906.

[2] Humphreys, *ibid*, **24**, 253, 1906.

[3] W. B. Anderson, *ibid.*, **24**, 221, 1906.

and therefore, even if the shift is a linear function of the inductive capacity, it would not be easy to prove it by experiment.

Anderson states that in the case of spark spectra pressure-shifts are greater in carbon dioxide than in hydrogen, and gives a table[1] of such lines. This table, all that he gives on this subject, does not appear definitely to prove the above statement. Only four lines admit of comparison, and of these, when reduced to the same pressure, two have greater measured shifts in hydrogen and the other two greater in carbon dioxide.

There is evidence of a typographical error in the data for one of the lines in hydrogen, but, leaving this line out, there is still one line with greater measured shift in hydrogen to two greater in carbon dioxide. Besides, the difference in measurements, when reduced to the same pressure, of a line in carbon dioxide is of the same order of magnitude as the difference between measurements on the same line in carbon dioxide and hydrogen. Finally, from the widths of the reversals, these lines could not be measured with much accuracy.

Therefore, in spite of this excellent paper, the influence of specific inductive capacity on pressure-shift does not appear to be well established.

In this connection it is interesting to compare arc- and spark-shifts,[2] under definite, though unavoidably different, conditions.

Table II gives this comparison for a number of iron lines. The first column gives the wave-length; the second, the average of several measurements of shift of spark lines (the inductance being 75 millihenrys and the capacity 0.0270 microfarads), produced in carbon dioxide at 50 atmospheres pressure; the third, the average corresponding shifts, computed for the same pressure from all measurements, of the arc lines produced in air by a direct current of about 15 amperes.

For another plate Anderson gives decidedly greater measured shifts, but in this case the reversals were much wider, and therefore, if I may judge from similar plates of my own, very deceptive, giving larger measured shifts than do the same lines produced under the same conditions, but with longer exposures, and therefore narrower reversals. It will be noticed that the general agreement is

[1] *Loc. cit.*, **24**, 252, 1906.

[2] Anderson, *loc. cit.*, p. 250.

TABLE II

SHIFTS, IN ÅNGSTRÖM UNITS, AT FIFTY ATMOSPHERES PRESSURE OF SPARK IRON LINES IN CARBON DIOXIDE AND OF THE SAME ARC LINES IN AIR

λ	Δλ		λ	Δλ	
	Spark Anderson	Arc Humphreys		Spark Anderson	Arc Humphreys
3687.58	0.107	0.096	3865.65	0.075	0.124
3709.37	.119	.106	3969.34	.092	.105
3758.36	.089	.099	3977.83	.075	.053
3763.90	.082	.112	3997.49	.079	.052
3765.66	.097	.102	4021.96	.093	.050
3767.31	.115	.110	4045.90	.106	.114
3805.47	.068	.107	4063.63	.105	.115
3813.12	.074	.070	4071.79	.131	.122
3815.97	.104	.114	4132.15	.107	.136
3824.58	.052	.040	4156.88	.073	.072
3827.96	.126	.113	4199.19	.090	.089
3834.37	.095	.117	4219.47	.097	.080
3856.49	.055	.040			

fairly close, showing that presumably neither the nature of the surrounding gas nor the mode of rendering the atoms luminous changes very greatly the magnitude of the shift. However, in the *Publications of the Yerkes Observatory*, Volume III, Part II, Plate XXII, Hale and Kent give a comparison between displacements they obtained with high-potential discharge in gases and those I obtained with the arc under similar conditions. Five of the lines agree reasonably well, three show much greater displacements when produced by sparks, while one, λ 3606.85, was not formerly measured by myself and is therefore left uncompared.

The present paper leaves these relations exactly as they were, except that λ 3606.85 has now been measured in the arc under pressure, and gives a disagreement even greater than does any one of the other compared lines, so that the possible difference of pressure-shift of arc and spark lines is still an open question.

In regard to the atom and its mode of vibration, the solution of which is one of the great aims of spectroscopic work, it seems that the Zeeman phenomenon demands that, whatever its substance, it shall, in part at least, be electrical; that the spectrum lines are not caused by simple mechanical vibrations of uncharged bodies such as are produced by an elastic solid; while the pressure-shift shows that

specific inductive capacity has but little, if any, influence on the period of whatever it is to which these lines are due.

As pointed out in a former paper,[1] since the spectrum lines of a radiant atom of whatever element are affected by a magnetic field, it seems certain that the atom itself must possess a magnetic field of its own, and therefore affect, in the Zeeman sense, the lines produced by its neighboring atoms, whether of the same or of different elements, and in turn suffer similar effects from them. It can be shown that the "Saturnian," and therefore magnetic, atom will, when placed in an external magnetic field, produce, as a result of induction, the Zeeman phenomenon; and also that a number of such atoms, when close together, will, through their mutual inductions, produce the phenomenon of pressure-shift, besides materially adding to the width of their spectrum lines.

The experimental discovery of either of these two phenomena, the Zeeman and the pressure-shift, might, in my opinion (since I believe them to be essentially the same thing), have led to the prediction of the other. As a matter of fact, the more obscure one, the pressure-shift, was first discovered, but its explanation in terms of interacting "Saturnian" or magnetic atoms did not suggest itself till a number of years after the independent discovery of the Zeeman effect.

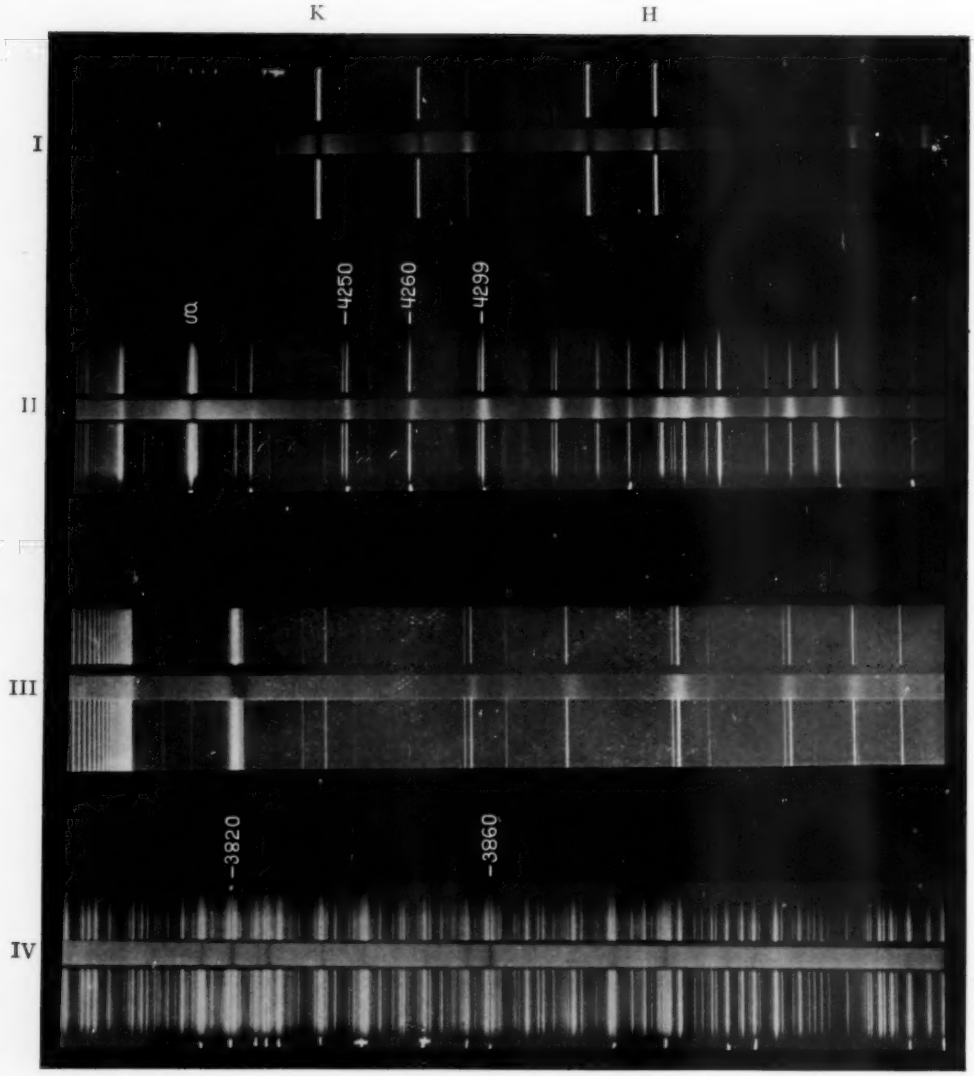
Since the displacements due to different pressures of spectrum lines can be measured, it is therefore possible to use these displacements in turn as measures of pressures even at places where no other method is available, and it may therefore serve to give some idea, among other things, of the depths in the sun at which different spectroscopic phenomena have their origin.

I am painfully aware that this paper does not include as many elements as it should, nor is it as exhaustive of any one as it profitably might be; but it is hoped that together with former papers enough is given to indicate what further work should be undertaken. I trust, too, that others may join in this investigation, since an exhaustive study of the subject will require much labor.

Much remains to be done, both on pressure-shift and on the Zeeman phenomenon, before the one-to-one relation between them

[1] Humphreys, *Astrophysical Journal*, **22**, 233, 1905.

PLATE IV



ARC SPECTRA UNDER HEAVY PRESSURE

can be definitely established or disproved. Besides, it is not improbable that a fuller study of pressure phenomena may lead to the discovery of groups and series of lines, either through their relative shifts, the pressures at which they cease to exist as lines, or other pressure effects. It may be possible to study the pressure effects on individual parts of a complex line as shown by the echelon or other powerful analyzer, and undoubtedly such an investigation would be of decided interest.

These, while not all, are among the more obvious pressure investigations I hope to see taken up and pushed to some definite conclusion.

The accompanying plate may be of some interest. In all cases the middle strip was taken at the high pressure and the sides at 1 atmosphere.

I shows in the third order and at 42 atmospheres, H and K, and between them two aluminium lines.

II. Second order, 69 atmospheres, gives *g*, shows the greatly displaced iron line $\lambda 4260.64$, and two iron lines, $\lambda 4250.31$ and $\lambda 4299.38$, that vanish under pressure.

III. Third order, 101 atmospheres, otherwise same as II.

IV. Second order, 101 atmospheres, ultra-violet iron lines.

This shows that many lines are still fairly good at this high pressure.

All the spectrum negatives used in this work were obtained in the physical laboratory of the University of Virginia, and I wish here to thank President Alderman and Professor Smith for their kindness in placing its excellent equipment at my disposal.

MOUNT WEATHER OBSERVATORY
BLUEMONT, VA.
May, 1907

APPARATUS FOR OBTAINING ELECTRIC ARCS UNDER HEAVY PRESSURE

By W. J. HUMPHREYS

A difference of potential of 110 volts is sufficient to maintain a fairly constant arc between carbon poles, either pure or mixed with metallic salts, provided the surrounding pressure is not greater than 10 to 12 atmospheres. With a difference of 220 volts the difficulty of maintaining a steady arc between fixed poles does not become excessive until a pressure of something like 25 atmospheres is reached. Higher voltages of course will produce good arcs with still greater pressures, but insulation becomes somewhat more troublesome; besides, when the apparatus must be worked with in the dark and one has to take chances on getting an occasional shock, high voltages are by no means desirable. Finally, the above being the usual commercial voltages, higher ones often are not easily obtained.

By making one of the poles in the form of a disk and rotating it while the other, in the shape of a rod or bar is brought up so as to touch it some distance from the center, an arc may be maintained with 220 volts between carbons, or carbon and metal, at much higher pressures. I have repeatedly used such an arc in spectroscopic work at 101 atmospheres, and found it still moderately constant and satisfactory.

The apparatus that proved efficient for this purpose is shown in section in Fig. 1, and mounted as in actual use in Fig. 2.

Referring to Fig. 1, the large vessel, *1*, is a forged cylindrical steel shell 6 inches (15 cm) in diameter and 16 inches (40 cm) long, both inside dimensions, with walls $1\frac{1}{2}$ inches ($3\frac{3}{4}$ cm) thick. A heavy walled block, *2*, is screwed into the side of this cylinder and made air-tight. A thick piece of quartz, *4*, is held tightly against the end of the tube, *2*, by means of the cap, *3*. Quartz is used to let ultraviolet radiation through, and besides it has the advantage of being very strong. A thin rubber or leather ring is placed between the quartz block and the end of the tube, *2;* and by means of a spanner-wrench, the cap, *3*, can safely be screwed up till the leak past the pack-

ing ring is negligible. It is necessary to cement the quartz block into its seat in the cap, *3*, to prevent any trace of leak passing by its outer face. If this is not done and a slight leak occurs — which is reasonably certain to happen — the outer face of the quartz will quickly be covered by moisture, and the beam of light seriously interfered with. That variety of cement known as "Khotinsky, hard" was used. It is easily applied and entirely satisfactory. The ends of the cylinder are drawn out to a proper size, bored in line with each other, and threaded on the outside. Partitioned metal pieces, *5*, are fitted to each of these ends, and screwed down with a spanner-wrench till their partitions come in contact with the glands, *9*, as shown, and force them against the packings, *10*, as tight as is necessary to prevent objectionable leakage about the steel tubes, *14*, that carry the electrodes.

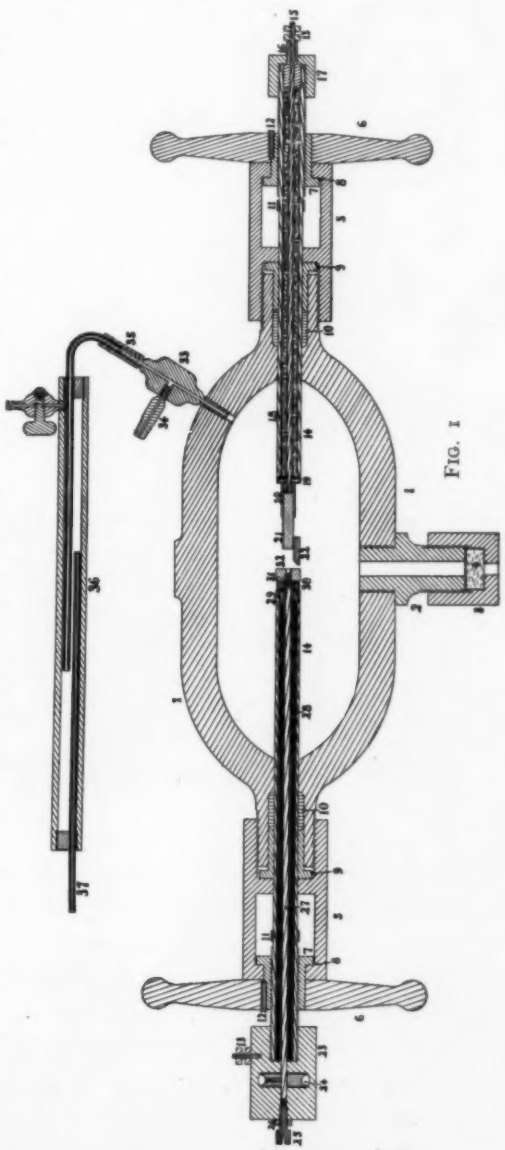


FIG. 1

In assembling the apparatus (in this particular both ends are alike) and before the tube, *14*, is introduced, the nut, *7*, is placed in the outer chamber of *5*, through an opening for that purpose, and slipped forward till shouldered against it at *8*. The hand-wheel, *6*,

is then fitted on this nut and made fast to it by means of the binding screw, *12*. The nut, *7*, is large enough to let the smooth part of *14* pass through it, and therefore when the pressure on the packing, *10*, is relieved by slightly unscrewing *5*, the electrodes may easily be removed from the cylinder; this operation is frequently necessary and therefore should be made as easy as possible. When in place, as shown, the electrodes are easily adjusted endwise by turning the hand-wheel, *6*, and therefore the nut, *7*.

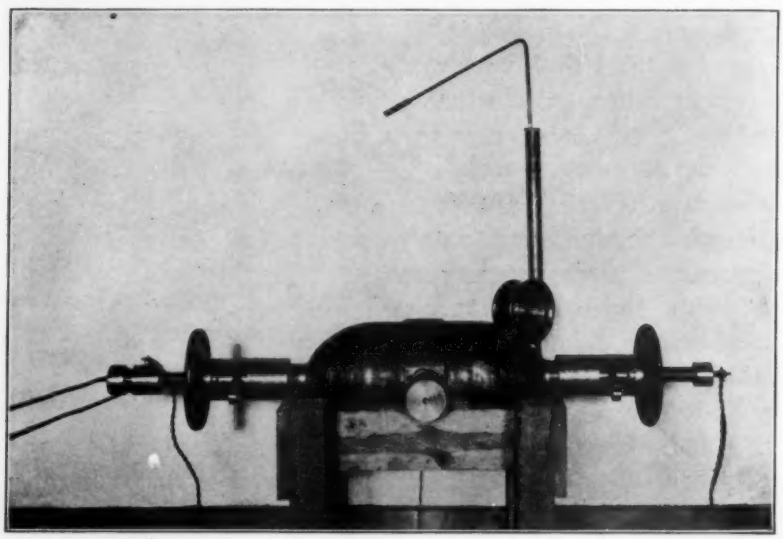


FIG. 2

A metal rod, *15*, provided with a binding post, *13*, is screwed through an ebonite block, *16*, which in turn is screwed into the tube, *14*, and further secured in place by the cap, *17*. The inner end of this rod passes through a mica block, *19*, against which the thimble, *20*, is tightly screwed. This thimble carries the fixed pole, which may be a carbon, or a metallic rod, beveled so as to touch eccentrically the rotating electrode, *32;* or it may carry a metal rod, *21*, to which the electrode, *22*, is made fast by any convenient method. Merely tying them together with copper wire is generally sufficient.

The space between the rod, *15*, and the wall of the tube, *14*, is filled with powdered or ground mica, *18*, which both insulates

electrically, and prevents absolutely any air leakage through the tube.

To prevent possible damage to the block of mica, *19*, from inequalities of pressure on opposite sides of it, a few small holes were bored very near it through the tube, *14*.

The rotating pole, *32*, is carried on the end of a small shaft, *27*, which is turned by means of a light round belt running from the pulley, *24*, to a suitable motor. This shaft passes through a close bearing in the cap, *23*, and the end-thrust is taken by a steel ball between the cupped ends of the shaft and the screw, *25*. The inner end of the shaft passes through a loose bearing in the metal block, *29*, and is screwed into the thick metal disk, *30*. A short rod, *31*, is also screwed into the disk, *30*, and to its other end is screwed the rotating pole, usually of carbon. The direction of rotation is such that friction against the pole, *22*, will tighten the pole, *32*. The short rod, *31*, is desirable because it is much more easily replaced than the shaft would be, and occasionally it gets badly injured as the rotating disk is burned away. The shaft is adjusted endwise by means of the screw, *25*, and when properly placed is made secure with the lock-nut, *26*.

The space between the rotating shaft and the wall of the tube, *14*, that carries it is filled with Ceylon graphite which is micaceous in its structure, and serves to help conduct the current taken in at the binding post, *13*, on *23*, to the shaft, *27*. Besides, it prevents almost absolutely any air-leakage through the tube, since any opening tends at once to be filled by graphite which is blown into it; and finally, no matter how tightly packed, the graphite is still a lubricant. Even when the pressure was 101 atmospheres the shaft was rotated with the greatest ease.

A fixed arc may be had, when the pressure is not too high, either by not turning the disk, or by making both poles of the fixed type, in which case there is no occasion to use poles placed eccentrically like *22*.

The gas from the compressor enters by means of the tube, *37*, into the liquid separator, *36*, and thence as shown into the shell containing the arc. The place of admission must be so situated and directed that no moisture shall be blown on to the quartz block, *4*, and

the draft shall not strike the arc itself. The first condition is always necessary, and the second desirable in the case of long exposures, since the pump must then be kept running with its blow-off valve set, in order to maintain a constant pressure shown by a gage attached to *34*. The pressure is let off by opening the stop-cock on the separator, *36*, which at the same time gets rid of any accumulated liquid.

In use the length of the shell is placed at right angles to the length of the slit of the spectroscope; and the separator, *36*, of course stands vertical.

It might seem that there would be trouble in seeing the poles, and therefore in properly adjusting them; but this is easily done by means of an incandescent electric light and an ophthalmoscope mirror. In making such adjustments it is most convenient to have the cap, *3*, removed.

It is desirable, when making the endwise adjustments of the poles, that the fixed one at least shall not rotate as a result of turning the nut, *7*. This trouble, though in practice it seldom happened, can be prevented by a clamp shown at *11*, the outer end of which presses against the edge of the opening in *5*.

The apparatus may seem not entirely simple, but there were many conditions to be met, and as constructed it gave such satisfactory results that the labor of making it was amply justified. The only part difficult to obtain was the forged-steel shell; this was finally furnished in an excellent condition by the Janney Steinmetz Co., of Philadelphia.

The apparatus in its completed form was paid for in part by a grant made by the Rumford Committee of the American Academy of Arts and Sciences, to whom I wish to express my sincere appreciation.

MOUNT WEATHER OBSERVATORY
BLUEMONT, VA.
May, 1907

MODIFICATION IN THE APPEARANCE AND POSITION OF AN ABSORPTION BAND RESULTING FROM THE PRESENCE OF A FOREIGN GAS

By R. W. WOOD

In the course of a somewhat extended study of the fluorescence and other optical properties of mercury, I have found what appears to be indisputable evidence that the appearance, and even the apparent position, of an absorption band can be profoundly modified by the presence in the absorbing vapor of a chemically inert gas. Reciprocal actions between dissimilar molecules have been sought for a long time, and a number of phenomena have been observed which have been claimed to show that an emission spectrum can be modified by the presence of foreign molecules; but objections can be raised in practically every case thus far recorded.

Professor Kayser, in his *Spectroscopy*, Vol. 2, p. 250, says:

> Ich glaube nicht, dass ein einziger dieser Versuche zu dem Schlusse zwingt, dass die Schwingungen eines Moleculs geändert werden durch Zusammenstösse mit fremden Moleculen, sondern dass es sich in allen diesen Fällen um chemische Wirkungen, um geänderte Art oder Vertheilung der elektrischen Entladung oder um veränderte Temperatur handelt.

I have submitted the photographs, which illustrate this paper, to Professor Kayser, and he has written me that he regards them as the first conclusive evidence that the collision of a molecule with a dissimilar one may affect its vibrations in a manner different from that resulting from collision with a similar molecule.

The vapor of mercury shows strong selective absorption in the ultra-violet. There is a very heavy band at λ 2536, in the neighborhood of which powerful anomalous dispersion occurs, and a weaker, less sharply defined band at λ 2350, which does not appear to influence the dispersion to any great degree. It is with the first of these two bands that we are chiefly concerned. With small vapor-densities we have in fact two very narrow lines, a strong one at λ 2536 and a weaker one at λ 2539, the two reminding one strongly of the D lines.

If a drop of mercury is placed in a small quartz bulb, which is

thoroughly exhausted and sealed, the appearance of the absorption spectrum for different vapor-densities is shown in Fig. 1, Plate V. A condenser discharge between cadmium electrodes was used as a source of light, as the continuous background is of considerable intensity, and the bright lines form useful reference points.

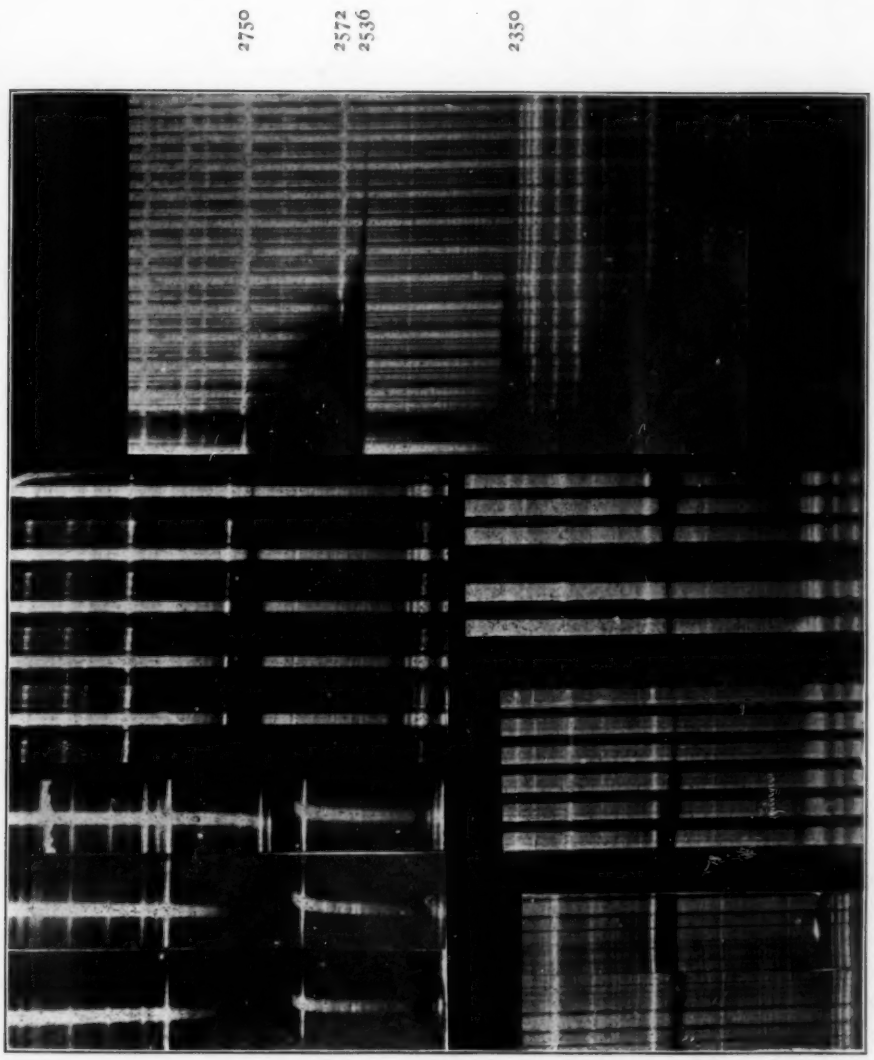
The bulb was placed in an air-bath, the temperature of which was gradually raised, and the spectra were photographed in succession with a small quartz spectrograph. This method enables us to secure a very good representation of the form of the absorption curve. The band at λ 2536 is seen to be extremely unsymmetrical. It widens out toward the red over a range of 400 Å.U., while its spread in the opposite direction is confined to a range not greater than 4 or 5 Å. U. Its widening is unsymmetrical from the very start, and in fact the spread toward the region of shorter wave-lengths becomes apparent only when the vapor has acquired considerable density. The temperature was well above a red heat at the end of the series, and the pressure was therefore several atmospheres. I mention this, as it proves that the effect of an atmosphere of hydrogen or air is not a mere *pressure* effect.

If the bulb is now opened and re-sealed, we obtain a series of spectra similar to those reproduced in Fig. 2. The band now widens symmetrically at first, reaching, however, a stage at which extension toward the region of shorter wave-lengths ceases. This symmetrical spread of the band is shown to still better advantage in Fig. 3.

This action of an inert gas in modifying the appearance of the absorption band was first observed in the photograph reproduced in Fig. 4. The fluorescence of the vapor was being studied by heating the metal in a quartz bulb provided with a long stem, in which the vapor was condensed. It was found in the first place that, if the stem was connected to an air-pump and the bulb exhausted, no fluorescence could be obtained, even though the metal was boiling briskly. On again admitting the air and repeating the experiment, a bright fluorescence was obtained.

Hartley, who first observed the fluorescence of the vapor, was of the opinion that it is necessary to have the vapor continuously formed—that is, the metal must be boiling and the vapor condensing on the walls—if continued fluorescence is to be observed. This may at first

PLATE V



Absorption Spectra of Mercury Showing Influence of Foreign Gas on Appearance of Absorption Band

sight appear to be true; for if we seal up a bulb and heat it far above the boiling-point of the metal, no trace of fluorescence appears. The cause of these apparently contradictory results was soon found. The reason why fluorescence does not appear when the bulb is connected to the air-pump and exhausted is that the vapor never acquires a sufficient density under these conditions. If we admit the air, the fluorescence does not appear until the metal has been boiling actively for ten or fifteen seconds, or until all of the air has been driven out of the bulb, which is then filled with pure vapor of mercury at a pressure of one atmosphere. A mixture of air and mercury vapor will not fluoresce, which explains the failure of the sealed bulb to show the phenomenon. If the bulb is first exhausted and then sealed, a very brilliant fluorescence is obtained.

The change in the appearance of the absorption band caused by the admixture of the air is very strikingly shown in Fig. 4. The spectra were taken in succession, the light of the spark having been passed through the quartz bulb containing the mercury. The flame which heated the bulb was gradually raised through the progress of the experiment. At first we have a mixture of air and mercury-vapor in the bulb, and the band broadens symmetrically. As the temperature rises and the boiling increases in violence, the air is gradually driven out of the bulb, the band actually *contracting* on the short wave-length side, notwithstanding the fact that the *vapor is actually becoming denser all the time.*

It was with a photograph taken in this way that the phenomenon was first discovered, the appearance being quite puzzling at first. The first explanation which occurred to me was that anomalous dispersion might have something to do with the phenomenon, for I had already made some experiments on the dispersion of the vapor at this point (photographs illustrating the effect at the band are reproduced in Fig. 5). In the first experiments the light from a small cadmium spark, after passage through the bulb, was brought to a focus on the slit of the quartz spectrograph. Under these conditions the effects described by Julius[1] might easily occur. Great care was therefore taken to exclude all possibility that dispersion might come into play. Small bulbs containing only a small speck

[1] *Astrophysical Journal*, **25**, 95, 1907.

of mercury, which could be completely vaporized, were sealed up either exhausted or filled with air, and the light from the spark was allowed to pass through them into the slit without the intervention of a lens. The effect of the air on the appearance of the absorption band was the same as before.

As can be seen from Fig. 4, an apparent shift in the position of an absorption band may result from the admixture of a foreign vapor, the shift, however, being only of the order of magnitude of the width of the band. It seems quite possible that what takes place on a large scale in the case of mercury vapor may take place on a much smaller scale in the case of other absorption lines. In Fig. 6 the upper spectrum is of mercury *in vacuo;* the lower, of mercury in hydrogen.

Sufficient data have not been obtained to make it possible to formulate a theory of the action. The obliteration of the fluorescence by the presence of the air or other gas is very suggestive; for the same thing has been found in the case of sodium vapor, iodine, anthracene, and other fluorescent vapors. In the case of anthracene, one of my students, Mr. Elston, found that, while the fluorescence was destroyed by air, oxygen, sulphur dioxide, and other more or less chemically active gases, it persisted in nitrogen and hydrogen.[1] This lead me to believe that the action might be a sort of incipient chemical action.

I therefore tried mercury-vapor and nitrogen, but the action was the same. To reduce the probability that anything akin to chemical action was responsible, it seemed worth while to try the effect of helium. A bulb containing a drop of mercury was filled with helium for me by Dr. Adams, of Princeton, who happened to have a quantity of the gas on hand. The bulb was sealed at atmospheric pressure, and the absorption spectrum photographed. The appearance of the band was the same as in the case of the bulb filled with air.

It is, of course, of the greatest importance to determine the effect of increasing the pressure of the air, comparing the spectrum obtained with a very long tube filled with air at atmospheric pressure and mercury at, say, 5 cm partial pressure, with the spectrum of a short column of dense mercury-vapor, also mixed with air at 1 atmosphere. The effects observed when the pressure of the air is raised to several atmospheres must also be carefully studied. In all probability a

[1] *Astrophysical Journal,* **25**, 155, 1907.

further spread of the band toward the region of shorter wave-lengths can be obtained by increasing the pressure of the inert gas in the bulb. Very likely the same result can be obtained with a very long tube with air at atmospheric pressure. In this way it will be possible to learn whether a given amount of the inert gas will affect only a given amount of the mercury vapor.

I have looked for a similar effect at the D lines of sodium, but thus far no evidence of an analogous shift has been observed. One of my students is at the present time engaged upon the study of the effect of inert gases upon the absorption spectrum of sodium. The appearance of the channeled absorption spectrum is profoundly modified, as I have already shown.[1]

THE JOHNS HOPKINS UNIVERSITY
June 7, 1907

[1] "Fluorescence and Magnetic Rotation Spectra of Sodium Vapor and Their Analysis," *Phil. Mag.*, (6) **12**, 499, 1906.

THE ABSENCE OF VERY LONG WAVES FROM THE SUN'S SPECTRUM[1]

By E. F. NICHOLS

During a visit to the Carnegie Solar Observatory in August, 1906, the writer set up a sensitive radiometer and reflected a beam of solar rays on five surfaces of rock-salt in succession. This procedure, according to the method of "residual rays,"[2] is known to give only a narrow spectral region, the mean wave-length of which is 510,000 Ångström units.[3]

The radiometer was of a type earlier described,[4] and the window which admitted the beams from the salt surfaces was a plate of quartz 0.5 mm thick, which has been shown to transmit about 60 per cent. of the energy in this part of the spectrum.[5]

The diagram (Fig. 1) shows the arrangement of apparatus. A beam of sunlight S reflected into the room from a silvered heliostat mirror fell upon a concave silvered mirror M_1 of 26 cm aperture and 75 cm focal length. The converging beam after leaving the mirror M_1 was reflected upon the five polished rock-salt surfaces s_1, s_2, s_3, s_4, s_5, in succession, of which the last surface s_5 was in focus of M_1. After leaving the surface s_5 the diverging pencil fell upon a small silvered collecting mirror M_2, and a secondary solar image was thus formed on one of the radiometer waves a. Exposures were made by raising and lowering a cardboard screen at A.

The solar beam was compared with one from an electric arc lamp L which, with its silvered converging mirror M_3, of 21 cm aperture and 100 cm focal length, could be moved into the position shown by the dotted lines. In this position a beam from the arc traversed the same path as the solar beam with which it was exchanged.

The beam from the arc caused a deflection of the radiometer suspension of 20 scale-divisions, but when a rock-salt plate $2\frac{1}{2}$ cm thick

[1] Contributions from the Solar Observatory, No. 19

[2] H. Rubens and E. F. Nichols, *Phys. Rev.*, **4**, 314, and **5**, 152, 1897.

[3] H. Rubens and E. Aschkinass, *Wied. Ann.*, **65**, 247, 1898.

[4] E. F. Nichols, *Phys. Rev.*, **4**, 298, 1897; *Astrophysical Journal*, **13**, 104, 1901.

[5] H. Rubens and E. Aschkinass, *loc. cit*, p. 249.

was interposed at *B*, no noticeable deflection could be detected. Rock-salt is very transparent to the region of the spectrum which comprises over 96 per cent. of the total energy emitted by the arc lamp, but is entirely opaque to the residual rays from rock-salt.

The test proved that over 99 per cent. of the energy of the beam under examination lay immediately in the spectral region about 510,000 Å. U.

The solar beam, however, gave a deflection of 17 scale-divisions, which was reduced only by 20 per cent. on interposing the test-plate. As about 10 per cent. of the reduction was due to the regular reflection at the two surfaces of the plate, not more than 10 per cent. could

FIG. 1

be attributed to absorption proper. As the test-plate surfaces were poor, it is quite possible that no interior absorption took place. On this assumption the experiment shows that *no energy of a wave-length near 510,000 Å. U. reaches the earth from the sun.*

If, on the contrary, we assume that the questionable 10 per cent. was all due to absorption of long waves, and not to a scattering of shorter waves at the imperfect surfaces of the test-plate, several factors must be taken into account to get at the amount of long-wave absorption in the atmosphere. The approximation will necessarily be a rough one, based on the relative concentrations of the beams from the two sources compared, and their relative temperatures rated as ideal black bodies. In this way it appears that *the maximum atmospheric transmission for the rock-salt waves cannot be greater than 3 per cent.*

In 1897 Rubens and Aschkinass[1] found that the energy of a solar beam after four successive reflections on fluorite surfaces was reduced to zero, thus proving that the opacity of the combined solar and terrestrial envelopes was practically total for wave-lengths near 240,000. At the same time they studied the absorption of water-vapor between the wave-lengths 70,000 and 200,000 and found practical extinction at the latter wave-length. In a paragraph of a later paper, which had until recently escaped my notice, the same writers found water-vapor an absorber of the residual rays from rock-salt and sylvite.[2]

Thus we may assume that the absence of waves 510,000 long in the sun's spectrum to be due to aqueous absorption, even though the tests described in the present paper were made at an elevation of nearly 6,000 feet in a dry climate.

The rôle of water-vapor as an absorber in the earlier infra-red spectrum has long been known, and the later experiments indicate an enormous extension of the region in which the vapor is active. Furthermore, water still shows a strong absorption and some dispersion, even for the shorter electric waves ($\lambda = 6$ to 10 mm). Thus the range of absorption of water, liquid, and vapor is surpassed only among the metallic conductors.

I am greatly indebted to Director George E. Hale for his kindly and whole-hearted co-operation in the present work, and I am no less indebted to the kindness of Dr. Henry G. Gale for constant assistance during the course of the experiments.

COLUMBIA UNIVERSITY, NEW YORK
May, 1907

[1] *Wied. Ann.*, **64**, 584, 1897.

[2] *Ibid.*, **65**, 251, 1898.

EXPERIMENTAL TEST OF DOPPLER'S PRINCIPLE FOR LIGHT-RAYS[1]

By PRINCE B. GALITZIN and J. WILIP

The first attempt to test Doppler's principle for light-rays in the laboratory was made by A. Bélopolsky.[2] For this purpose he constructed a special apparatus, consisting of two systems of light wheels coupled in pairs. Each pair carried eight mirrors mounted near the periphery of the wheels. Special electric motors set the two systems of eight mirrors in very rapid rotation in opposite directions. These wheels with mirrors were so arranged that a light-ray falling upon them would undergo several reflections from the silvered glass surfaces. By inclination of the direction of the incident beam the number of reflections could be varied at will.

If λ represents the wave-length of the incident ray, v_1 the linear velocity of the center of the mirror, V the velocity of light, and n the number of reflections, then, according to Doppler's principle, the wave-length of the incident beam after the nth reflection will have undergone a change $\delta\lambda$, which is represented with sufficiently close approximation by

$$\delta\lambda = \pm 2n\frac{v_1}{V}\lambda . \qquad (1)$$

If the systems of mirrors on the upper side of the wheels, where the reflection takes place, turn toward each other, the wave-length is shortened and the negative sign is employed; if the direction is opposite to this, the positive sign is used.

A detailed description of the apparatus and the method of its use in testing Doppler's principle is given in the communication by Bélopolsky cited above. Consequently we shall content ourselves with referring to that paper.

Bélopolsky used sunlight as the source of light in his investigation. For the dispersion in the spectrograph two compound prisms were

[1] Translated from *Bulletin de l'Academie impériale des Sciences de St. Pétersbourg*, (6) 1907, No. 8, May 1.

[2] *Bulletin de l'Academie des Sciences de St. Pétersbourg*, XIII, No. 5, 461, 1900; *Astrophysical Journal*, 13, 15, 1901.

used. The photographic exposures were made in the region of the spectrum from λ 4380 to λ 4500. The displacement of several lines on each plate was measured; from these the mean displacement was deduced and then the corresponding velocity in the direction of the beam was computed.

Since the apparatus used by Bélopolsky possessed no very great dispersion, and the displacements were extremely small, even after the sixfold reflection which he employed, these measurements could make claim to no great accuracy. In fact, from the measurement of some lines the displacement was opposite to that expected from Doppler's principle, but nevertheless the mean value in every case gave a displacement which represented correctly the direction of rotation of the mirror.

Bélopolsky secured six different series of observations and compared the velocities derived with those obtained directly from the number of rotations of the wheels.

Considering the comparatively crude means with which the investigation was conducted, the agreement of the values may be considered quite satisfactory. With such dispersion only a skilful observer like Bélopolsky could obtain such good results.

Since Bélopolsky considered the investigation only as a first trial, it seemed to us very desirable to repeat the same experiment with more powerful means, employing the large Michelson echelon spectroscope of the Physical Laboratory of the Academy of Sciences, which possesses such a powerful dispersion. Bélopolsky kindly loaned to us the apparatus with the rotating mirrors, and with this we conducted a number of experiments which we now describe.

The theory of the echelon and the various methods of application of this valuable instrument have been worked out and examined by one of us,[1] and at that time its eventual application to the examination of Doppler's principle was spoken of. Consequently we refer to that paper in the discussion which follows.

We used an Arons' mercury arc lamp as the light-source. This was fed with a nine-ampere, but later thirteen-ampere, current from

[1] See Fürst B. Galitzin, "Zur Theorie des Stufenspectroscops," *Bulletin de l'Academie Impériale des Sciences de St. Pétersbourg*, V^e Série, T. XXIII, Nos. 1 and 2, p. 67, 1905.

the electric mains of the Academy of Sciences. After several successive reflections the beam was concentrated by the lenses upon the slit of the echelon spectroscope. Two mercury lines, that in the green at λ 5461 and that in the indigo at λ 4358, were photographed after passage through the echelon. For these exposures, the lower half of the collimator slit was first occulted by means of a screen mounted independently of the spectroscope, and an exposure was made with the mirrors rotating. Then the upper half was covered and a second exposure was made with the mirrors rotating in the opposite direction. The movement of the screen was so regulated that there was only a very small space between the ends of the two halves of the lines. After developing and drying the plates the displacement, $2\ \delta m$, of the two halves of the lines with reference to each other was measured under a microscope. $2\ \delta m$ is given in divisions of the head of the screw of the ocular-micrometer, a division of which is equal to $\frac{1}{400}$ mm. This displacement represented a double velocity in the direction of the light-ray.[1]

The exposures were always secured on the side of the echelon of the greater dispersion and in spectra of various orders. It is known that for one and the same line in neighboring orders of the echelon the difference in wave-length, $\Delta\lambda$, is independent of the order of the spectrum. If we represent the separation of the same lines on the photographic plate by Δm, the change in wave-length, $\delta\lambda$, corresponding to the displacement, δm, of the lines for the rotating mirrors relative to the stationary is expressed by

$$\delta\lambda = \delta m \frac{\Delta\lambda}{\Delta m}. \qquad (2)$$

When $\delta\lambda$ is determined, the desired velocity can at once be easily computed. It is, regardless of sign,

$$v = \frac{\delta\lambda}{\lambda} V. \qquad (3)$$

In this manner the velocity is expressed in terms of displacement of the lines.

The same quantity may be determined from the number of rotations per second of the wheels carrying the mirrors. Let r represent

[1] The ends of the half-lines lay so near to each other, that in the measurement of the displacement the influence of the curvature of the lines could be entirely neglected.

the distance of the middle of the small mirrors, each 2 cm wide, from the axis of rotation, and v_1 the linear velocity of the center of the mirrors, then

$$v_1 = 2\pi N r .$$

At the nth reflection

$$v = 2nv_1, \tag{4}$$

or

$$v = 4n\pi N r. \tag{5}$$

The test of Doppler's principle consists in comparing the values of v computed from the formulae (3) and (5).

The distance r was obtained by direct measurement. It is

$$r = 0.112 \text{ meter.}$$

A special speed-counter from a mercury interrupter was coupled with the rotating-mirror apparatus to determine the number of rotations N of the wheels corresponding to various exposures. The conversion factor was previously obtained by a series of experiments with a common revolution-counter and a Löbner second-counter which permitted hundredths of a second to be read off.

In all cases we sought to give the wheels the highest possible velocity. This was achieved with a current of about 7.3 amperes. The mean number of rotations per second varied for various series between $N = 41.1$ and $N = 46.2$, which represent linear velocities of the centers of the mirrors from 28.9 to 32.5 meters per second. During single series, for the same direction of rotation of the mirrors, N remained very constant.

Before the investigation was begun, the outer surfaces of the mirrors were carefully silvered by a special method.

The photographic exposures were made in part on Edwards' isochromatic plates and partly on Seed's extra-rapid plates.

At first we wished to photograph, together with the green and indigo-blue lines, the yellow line at λ 5791. But trial exposures showed that the exposure-time necessary to obtain sharp and well-measurable lines with the mirrors rotating was too long; and, since for long intervals of time we could not be sure of maintaining the echelon at sufficiently constant temperature—an indispensable condition in these experiments, as we shall presently see—the yellow line

was given up. Indeed the line turned out to be superfluous, for the green and indigo-blue lines belong to regions of the spectrum sufficiently widely separated to give a quite extended test of Doppler's principle.

The exposure-time for the two lines employed was varied for different photographs. The longer the exposure-time, just so much sharper are the lines, and just so much easier is it to measure the relative displacement. On the other hand, too long exposures are dangerous on account of the possible variation of temperature.

After successive exposures with rotating mirrors, exposures were always made with stationary mirrors, on another portion of the plate, in order to determine the dispersion, or the value of Δm, for the given position of the echelon.

The value of Δm need not be measured with great accuracy; nevertheless we give Δm as the mean of six or more measurements, three always being made by each of us.

The chief emphasis of these tests lay in the determination of $2\delta m$. Each value of δm given below is the mean of 20 single measurements, 10 by each of us. We may mention that the agreement of the single values in general is entirely satisfactory and in no case did we get a negative result, that is, a deplacement which is not in agreement with Doppler's principle in respect to the direction of rotation of the mirrors. On the contrary, the measured displacements, as we shall see further on, correspond very well with the values to be expected from Doppler's principle, in view of the admissible errors of observation. Most of the exposures were obtained with a fourfold reflection of the rays, but exposures were also made with six reflections.

Let us now consider a little more closely the influence of a possible oscillation of temperature on the results of these measurements.

It is evident in advance that a change of temperature can be very disturbing, for the echelon in a certain sense may be regarded as a very sensitive interference refractometer, and consequently each variation of temperature, which changes the height of the steps and the index of refraction of the glass, produces a wandering of the fringes. Let us see how great an error a change in temperature of 0°.01 C. will produce in the velocity v derived from the displacement of the lines.

In the paper cited earlier, "Zur Theorie des Stufenspectroscops" (p. 117) is found the formula

$$\delta\psi\frac{n_2}{r}\left\{\ \delta\mu+(\mu-1)a\delta\tau\ \right\}, \tag{6}$$

which gives the angular displacement of a spectrum line for a change of temperature $\delta\tau°$ C.

μ is the index of refraction of the glass for a given spectrum line; $\delta\mu$, the change of μ, when the temperature increases $\delta\tau$ degrees; a is the linear coefficient of expansion of the glass, = 0.0000085; n_2 and r are two quantities which are defined by the formulae (26) and (29) (*loc. cit.*). Let m be the linear distance in divisions of the head of the ocular micrometer of the microscope corresponding to the angle ψ, then we may make

$$m=A\psi,$$

where A is a constant, dependent on the properties of the optical train.

If $\Delta\psi$ is the angular distance between two fringes of adjacent orders, then

$$\Delta m=A\Delta\psi.$$

According to the formula (36) (*loc. cit.*)

$$\Delta\psi=\frac{1}{r}.$$

If for brevity we set

$$\delta\mu+(\mu-1)a\delta\tau=s, \tag{7}$$

then

$$\delta m=n_2\Delta m s.$$

δm is also the error in the measured displacement $2\,\delta m$, in consequence of a change of temperature $\delta\tau$.

We can therefore place

$$\delta(2\delta m)=n_2\Delta m s\,.$$

It follows from formula (2) that

$$\delta(\delta\lambda)=\tfrac{1}{2}n_2\Delta\lambda s,$$

or, from equation (3),

$$\delta v=\tfrac{1}{2}n_2\frac{\Delta\lambda}{\lambda}Vs\,. \tag{8}$$

This very simple formula permits the error in v to be computed directly.

From the numerical data which are given in the paper to which we have referred, and the values of $\delta\mu$ for flint glass for various spectrum lines (from the tables of Landolt and Börnstein), we compute the following values for the constants contained in formula (8), calculating δv for a temperature-change of 0°.01 C.

	Green Line	Indigo-blue Line
λ	5461	4358
$\Delta\lambda$	0.4766	0.2859
n_2	18277	22901
μ	1.5781	1.5918
$\frac{\delta\mu}{\delta\tau}$	0.00000396	0.00000556
s	0.0000000887	0.0000001059 (for 0°.01 C.)
δv	0.021 km	0.024 km

We see that a change of temperature of only 0°.01 C. affects the velocity by 21 to 24 meters per second. Hence if an echelon is to be used in actually testing Doppler's principle, the observer must exercise the utmost care to keep the temperature constant during both exposures with rotating mirrors.

In practice this is indeed quite a difficult matter and at first gave us much trouble, but finally we overcame the difficulties and obtained a quite constant temperature during the consecutive exposures. For this purpose the echelon-spectroscope with all the accessories was inclosed in a glass case, and the interior, where a change of temperature was most to be feared, was filled with cotton. A layer of cotton was also put on the cover over the echelon. In addition to this the whole was mounted in the basement of the main building of the Academy of Sciences where the daily oscillation of temperature is very small, and here the windows were covered. A very sensitive thermometer, divided in fiftieths of degrees, whose bulb was near the echelon, gave extremely small variations. In spite of this the observations commonly had to be confined to the morning hours only, when the sun had not yet shone around the corner of the building; and even then only one line (two consecutive exposures) could be investigated on one and the same day because the air of the laboratory became disturbed by the rotation of the mirrors, as was indicated on the thermometer after a time. A small change of temperature at

the beginning of the observations is not so dangerous, because the poor conductivity of the glass makes it probable that the echelon assumes this temperature much later. But if the investigation is pushed farther one cannot be sure of the conditions of temperature in the echelon. In no case was the measured temperature-change greater than from 0°.01 to 0°.02 C., with a single exception where the change amounted to $3\frac{1}{2}$ hundredths.

By observing all these precautions, the results obtained were very satisfactory, as will be recognized in the summary of the investigation given below.

The numerical results are given in the following tables, I and II. The first gives the cases of fourfold reflection, the second of sixfold.

The first column gives the date of the observation; the second, the emission line employed; the third, the rotation number N. It should be remarked that N is the mean of four readings made at the beginning and end of the two consecutive exposures. The fourth column gives the exposure-time for each plate; the fifth the displacement sought (moving mirror relative to stationary mirror) in divisions of the head of the ocular micrometer ($2\delta m$, the sum of the two displacements, was measured directly[1]). The sixth column gives the value of Δm, that is, the distance of the two bands in neighboring orders, likewise in divisions of the head. The values of $\frac{\Delta\lambda}{\Delta m}$ are collected in the seventh column. These quantities give a measure of the dispersion of the apparatus; that is, the number of Ångström units corresponding to a division of the micrometer-head. In the eighth column are the velocities derived from the observations. The ninth gives the velocities computed by the number of rotations per second, N. Finally, the last column gives the difference Δv of these values $\{v$ (from number of rotations) $- v$ (from displacements)$\}$.

With reference to the determination of v from the number of rotations per second N, it should be stated that we undertook to mount the mirrors so that the rays reflected from the center of the mirrors, while the reflecting surface was parallel to the slit, should coincide as closely as possible with the middle of the slit, where the

[1] It is to be remarked that, in order to vary as much as possible the conditions of the experiments, the investigations were begun at one time with one direction of rotation, the next time with the opposite direction.

TABLE I

Date	Line	N	Exposure	δm	Δm	$\frac{\Delta\lambda}{\Delta m}$	v from Displacement	v from No. of Rotations	Δv
			mins.	divs.	divs.		km. per sec.	km. per sec.	km. per sec.
April 10.....	Green	45.1	15	4.75	524.9	0.000908 Å. U.	0.237	0.254	+0.017
" 11.....	Green	45.4	15	5.28	559.6	852	0.247	0.256	+0.009
" 12.....	Indigo-blue	46.2	30	6.24	399.3	716	0.308	0.260	−0.048
" 15.....	Indigo-blue	45.9	60	4.80	403.1	709	0.234	0.258	+0.024
" 16.....	Green	45.3	30	5.11	564.4	845	0.237	0.255	+0.018
" 17.....	Green	45.4	30	5.16	567.3	840	0.238	0.256	+0.018
" 18.....	Indigo-blue	45.5	50	6.02	429.3	666	0.276	0.256	−0.020
						Means	0.254	0.256	

TABLE II

Date	Line	N	Exposure	δm	Δm	$\frac{\Delta\lambda}{\Delta m}$	v from Displacement	v from No. of Rotations	Δv
			min.	divs.	divs.		km. per sec.	km. per sec.	km. per sec.
April 20.....	Green	45.0	60	7.60	491.0	0.000971 Å.U.	0.405	0.379	−0.026
" 21.....	Green	44.0	60	6.68	490.1	973	0.357	0.372	+0.015
" 22.....	Green	41.1	60	6.27	495.4	962	0.331	0.346	+0.015
						Means	0.364	0.366	

displacements are measured. In the computation of v, from formula (5), we have taken for r the distance of the center of the mirrors from the axis of rotation. If the reflection had taken place on one of the edges of the mirrors, the computed velocity would have been in error about 10 per cent.

When we consider the results in these two tables we see that the difference between the velocity v computed from the displacement and from the number of revolutions amounts in the mean to only about twenty meters per second.

Considering the difficulty of these measurements and the influence of variation of temperature mentioned above, the agreement can be considered as entirely satisfactory. Therefore, Doppler's principle for light-rays is, within the permissible range of error of observation, fully confirmed.

Minor Contributions and Notes

A PHOTOGRAPHIC STUDY OF THE SPECTRUM OF *SATURN*[1]

During the autumn of 1905 I photographed the spectrum of *Saturn* for the purpose of investigating the absorption of the planet's atmosphere. Plates specially sensitized to the orange-red were used. With these plates the spectrum was photographed as far down as to Fraunhofer's C. Since low dispersion is most effective in depicting faint bands, such as are found in the spectra of the planets, a single-prism spectrograph was employed.

The slit of the spectrograph was set upon the major axis of the Saturnian system, and the spectra of the ansae of the rings appear on the plates above and below the spectrum of the ball of the planet. The moon served as a source for the comparison spectrum, the two parts of which lie outside the spectra of the rings. In these relative positions the spectra from the different sources may readily be compared, and thus the detection of faint bands be facilitated.

The moon was photographed at about the same altitude as *Saturn*, so the absorption due to the earth's atmosphere affected equally both spectra. The solar light reaching the earth by reflection from the moon has the same spectrum as direct sunlight, and also the same as the sunlight that reaches *Saturn*. Therefore, if light from *Saturn* shows a different spectrum from that of the moon, these differences must be produced by selective absorption (and reflection) in the atmosphere of *Saturn*.

For this investigation of *Saturn's* spectrum, which is similar to the one made on the spectrum of *Jupiter*, published in *Bulletin* No. 16, a series of about ten photographs were made. Examination of these plates, under low magnification, revealed several absorption bands in the region between the Fraunhofer lines F and C. Three of the plates have been measured for the wave-lengths of the bands. The results of the examination and the measurements are summarized in the following table:

[1] *Lowell Observatory Bulletin* No. 27.

Wave-Length	Remarks
5430	Band in *Saturn*—not seen in rings. It is visible on all the plates as a fairly strong band.
5592	There is a suggestion on some of the plates that the solar band here may be strengthened by absorption in *Saturn*. However, this can be only slight. The same effect is noticeable on the plates of *Jupiter*.
577	Some of the plates indicate that *Saturn* has here a weak band which seems to cover the region between the solar bands at λ 5758 and λ 5785.
6145	There appears, on most of the plates, a narrow band here in *Saturn*. Although there are solar lines near, it is doubtful if they produce the band.
6193	A very strong band—the strongest one in the spectrum of *Saturn*. No trace of it is seen in the spectra of the rings. It is broad and symmetrical, and is traceable to the band at λ 6145.
645	Near the middle of a broad ill-defined band in *Saturn*.
6563	The solar line C. As far down as the spectrum can be examined.

In relative strength these bands stand in the following order:

λ	
6193	
5430	
6145	
645	
577	(Weak and doubtful.)

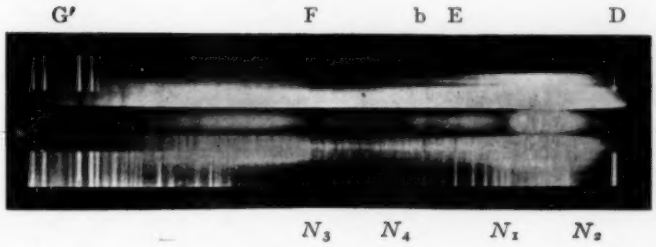
It is an interesting fact that none of the bands observed in the spectrum of *Saturn* has been seen in the spectra of the rings. That the faint ones have not been seen could be due to the difficulty of observing weak bands in spectra as narrow as those of the rings, but a band much weaker than the one at λ 6193 would be apparent. The absence of this heavy band from the rings shows that, if they possess an atmosphere at all, it must be much rarer than that surrounding the ball of the planet. The absence of this band from the ring spectra was observed visually by Keeler and photographically by Hale and Ellerman.

It should also be remarked that none of the absorption bands in the spectrum of *Saturn* can be identified with those bands due to absorption in the earth's atmosphere. My spectrograms show no trace of aqueous vapor absorption in *Saturn*.

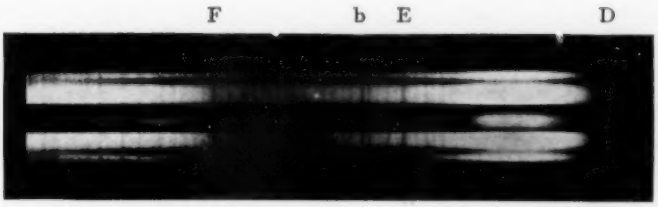
A fivefold direct enlargement of one of the plates of *Saturn* is reproduced in the accompanying plate, together with similar direct enlargements of plates of *Jupiter, Uranus, and Neptune*. A table is also

PLATE VI

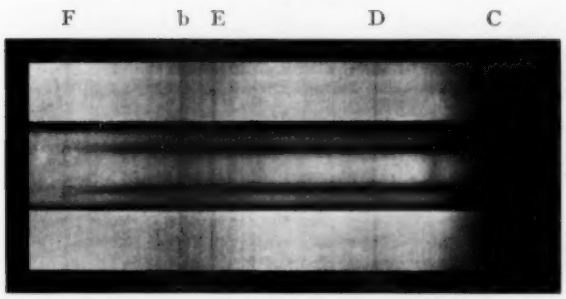
NEPTUNE



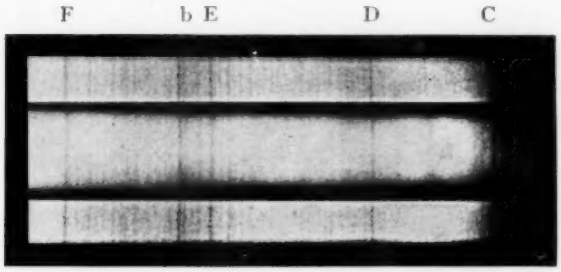
URANUS



SATURN



JUPITER



Band Near	Saturn		Jupiter		Uranus[1]		Neptune[1]	
λ	λ	Remarks	λ	Remarks	λ	Remarks	λ	Remarks
486					4861	F, stronger and broader than in Sun.	4861	A narrow, strong band.
510					5101	A weak band	5104	A strong band.
523							5225	Broad, weak band.
541			5406	Blue component, which is largely solar.	5404	Increased absorption; there are strong solar lines at this place.	5396 5408	Slight local absorption probably due to pair of solar lines.
			5427	Yellow component, which is largely Jovian.	5427	Maximum of strongest absorption band in spectrum.	5425	A secondary maximum in the strongest band in the spectrum.
543	5430	A fairly strong band					5432	The point of maximum intensity of this band.
559	559	A suggestion of absorption.						
577	577	What seems to be a weak band covering the region between the solar lines at 5759 and 5786.	5755 (5769) 5783	Green component. Center. Orange component.	5755 (5769) 5783	A first maximum in second strongest band in spectrum. A second maximum in this strong band	5771	Point of maximum intensity of violet (and stronger) component of band.
602			6023	Weak and doubtful.			5780	Maximum of red (and weaker) component.
615	6145	A narrow band.						
619	6193	The strongest band in this planet. It is not in the rings.	6192	Strongest band in spectrum.				
647	645	Near the middle of a broad, ill-defined band.	6437 (6465) 6495	Orange edge of bd. Center of band. Red edge of band.		(*Uranus* not photographed below D.)		(*Neptune* not photographed below D).

[1] *Lowell Observatory Bulletin* No. 13

added here showing in parallel columns the results of the study of the spectra of the four planets.

From a comparison of these plates and an inspection of the table it will be evident that the planets which are telescopically similar have similar spectra: the spectrum of *Jupiter* is similar to that of *Saturn*, while the spectrum of *Uranus* is more like that of *Neptune*. However, the bands at λ 543 and λ 6193 are stronger in *Saturn* than in *Jupiter*, whereas the band at λ 646, which is of considerable strength in *Jupiter*, is weak in *Saturn*. These disagreements suggest a difference in the relative proportions of the gases in the atmospheres of the two planets.

The spectrum of *Uranus* differs from those of *Jupiter* and *Saturn* in the increased strength of the bands at λ 543 and λ 577. In it the hydrogen line F is of increased strength and it contains in addition other faint bands not seen in *Saturn* and *Jupiter*. The comparison spectrum of the *Uranus* plate is that of *Saturn*, but as the exposure was not suited for bringing out the bands in *Saturn*, the plate cannot serve for comparing directly the two spectra. On this plate the spectra do not extend below the D lines.

The plate of *Neptune* shows the spectrum of this planet to contain many strong absorption bands. These bands are so pronounced in the part of the spectrum between the Fraunhofer lines F and D as to leave the solar spectrum unrecognizable. An iron-sodium spark and the solar type-star *β Geminorum* were photographed on this plate for comparison. The star spectrum lies above and below that of the planet, and has on its outside borders and extending beyond it the bright *Fe* and *Na* spark lines. *Neptune's* spectrum is strikingly different from that of *Uranus*, the bands in the latter planet all being reinforced in *Neptune*. In this planet there are also new bands which have not been observed in any of the other planets. The F line of hydrogen is remarkably dark. As has been stated before, this band is of more than solar strength in the spectrum of *Uranus* also. Thus free hydrogen seems to be present in the atmospheres of both these planets. This and the other dark bands in these planets bear evidence of an enveloping atmosphere of gases which is quite unlike that which surrounds the earth.

V. M. SLIPHER

JUNE 1906

PLATE VII

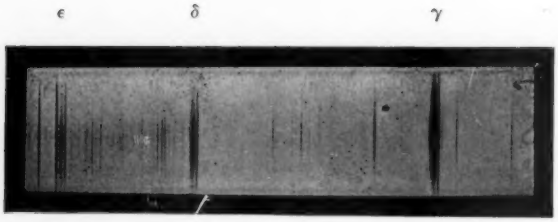


FIG. 1.— Canal Rays in Hydrogen

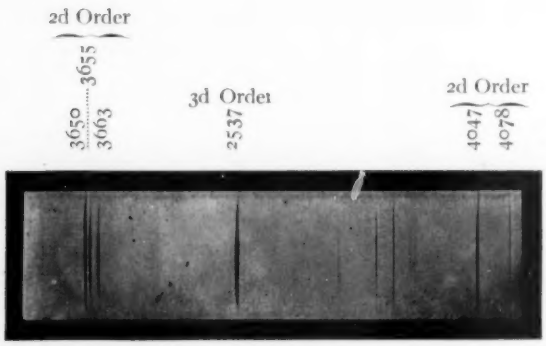


FIG 2 — Canal Rays in Mercury Vapor

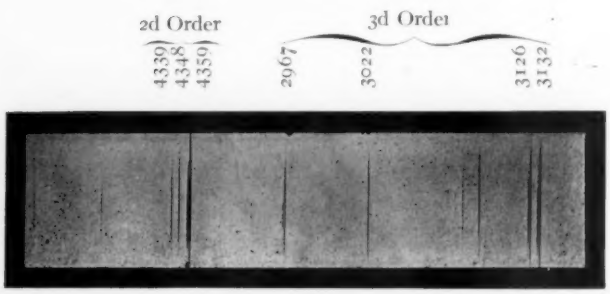


FIG. 3.— Canal Rays in Mercury Vapor

PHOTOGRAPHS OF THE DOPPLER EFFECT IN THE SPECTRUM OF HYDROGEN AND OF MERCURY. REJOINDER TO MR. HULL'S REPLY

I recently published in this *Journal* some remarks on a paper by Mr. G. F. Hull on the Doppler effect for canal rays. Mr. Hull's reply to these remarks does not seem to add clearness in regard to the question at issue; and as the matter seems to me important, it is probably best for me to publish my spectrograms as an answer to Mr. Hull's statements.

The three spectrograms of Plate VII were obtained with a Rowland concave grating of one meter's radius. The hydrogen lines of Fig. 1 were photographed in the second order; the lines of the other two spectrograms belong in part to the second, and in part to the third order of the mercury spectrum. In the case of hydrogen as well as of mercury, the canal rays moved toward the slit.[1] It will be seen that in both cases we obtain the lines at rest in their normal position; and simultaneously, on the side toward the ultra-violet, the Doppler effect —that is, a band of movable lines which were emitted with a different velocity by the particles of the canal rays. Between the unmoved and the moved lines lies a maximum of intensity.

The occurrence of the minimum of intensity—that is, the lack of an emission of light at a small velocity of translation—excites a great theoretical interest. Its breadth also gives a measure of the magnitude which the cathode-drop must have in order that the canal rays should assume so large a velocity that the movable intensity should become appreciable, hence that the Doppler effect should be obtained.

A second point of importance in judging as to the statements of Mr. Hull is the ratio of the movable intensity to the stationary intensity. The reproduction of the spectrograms will doubtless permit us to see that this ratio is large for hydrogen, but small for mercury lines. These also further exhibit very large differences among themselves, the ratio being the largest for the line at λ 2537, smaller at λ 4047, and very small at λ 4078. As I have already shown elsewhere, the movable intensity has its direct origin in the translation

[1] The reproductions were made from glass negatives which were contact copies obtained without any retouching. The original negatives were intensified with uranium, whence the coarseness of the silver grains.

of the particles of the canal rays; the stationary intensity probably comes to the point of emission at the separation of the negative electrons from neutral atoms, hence at the development of positive atomic ions ("Atomionen"). This ionization may be accomplished by the canal rays themselves, or by the secondary cathode rays which are produced from the canal rays according to the observations of J. J Thomson and Chr. Füchtbauer. It is a fact that for canal rays the stationary intensity is small, while it is large for canal rays in mercury vapor. This may be related to another phenomenon, namely, the fact that the slow cathode rays of the positive column of light of the glow-discharge at lower temperature brings the series-lines of mercury into more intense emission, but the series-lines of hydrogen only to a faint emission. By diminution of the absorption of the canal rays and the cathode rays in the space around the gas, or by the reduction of the gas-pressure, the stationary intensity in the spectrum of the canal rays may be diminished. The movable intensity can be increased by enlarging the velocity of translation.

In his reply Mr. Hull writes:

> Professor Stark suggests that it is necessary to satisfy the conditions that the cathode-drop producing the canal rays shall not be less than a certain limiting value. The inference is made that Professor Stark has found that limiting value. It certainly would have made for definiteness of our ideas if Professor Stark had given us the limiting values for hydrogen and mercury.

If Mr. Hull had attentively read my detailed paper which appeared in the *Annalen der Physik* in November 1906, his remark just quoted would have been superfluous, for I published the limiting values which the cathode-drop must exceed in order that the Doppler effect should be demonstrable in the hydrogen series, and for the mercury lines. It amounts for the hydrogen lines to about 700 volts, as Mr. Hull may see if he reads the article again, while it is of different magnitudes for the *Hg* lines—for λ 2537, 8000; for λ 4047, 7000; for λ 4078, 15,000 volts. I also gave approximate data as to the ratio of the movable intensity to the stationary intensity of the series lines produced by the canal rays. Table VIII of my paper shows how greatly this ratio depends on the gas-pressure and the cathode-drop. In referring to this table I should also like to make the following remark: If Mr. Hull is of the opinion that it is very easy to demon-

strate the Doppler effect for hydrogen, I agree with this view, and add that it is much easier still in case of mercury and helium to obtain the stationary lines from their canal rays without any indication of the Doppler effect.

Mr. Hull appears to attach more value to his negative results than to my positive results. Hence I may point out that Professor F. Paschen also obtained the Doppler effect for the *Hg* lines. He wrote me, in a letter dated November 10, 1906: "I have also obtained the Doppler effect in mercury at high potential;" and in a letter dated February 12, 1907, he says:

> I shall hardly publish my observations on mercury. If the Doppler effect is observable in mercury, I should have to see to it that I could satisfy the appropriate conditions for other cases. I contented myself with seeing the effect at λ 5461, and photographing it for several other lines. These observations were made only for my own information, and are not comparable with your extensive results, so that I should have to continue mine further if I wished to publish anything about them.

I suspected that Mr. Hull was unsuccessful in his search in case of mercury as well as helium, for the reason that the movable intensity is very small in comparison with the stationary intensity for the helium lines also. This is in fact the case, and Dr. Rau, of Braunschweig, has meanwhile succeeded in demonstrating the Doppler effect for the canal rays in helium. The reason for the predominance of the stationary intensity probably will also be related to the fact that the slow cathode rays in the positive column of light of the glow-discharge at low temperature bring the series lines of helium into intense emission.

As an explanation of the negative results of his experiments, Mr. Hull has advanced the hypothesis that the absence of the Doppler effect in mercury and helium is occasioned by the presence of other non-luminous particles, probably those of hydrogen, which are easily set in motion. This hypothesis is contradicted by the following fact: The Doppler effect is to be observed for quick canal rays in case of the mercury and helium lines, whether a considerable amount or only an extremely small amount of hydrogen is admixed with these gases.

J. STARK

HANNOVER
May 10, 1907

ARE LUMINOUS METALLIC PARTICLES THROWN OUT FROM THE POLES IN THE SPARK DISCHARGE?

Professor Schuster's criticism[1] of my article published in the January number of this *Journal* is due in part to a misinterpretation of my views. For this misinterpretation I feel that I am partly responsible, for upon again looking over those two sentences which he so severely criticizes I find that they are not satisfactorily worded. In them I tried to condense a rather extended argument, with a resulting loss in clearness. But had Professor Schuster, or the person of whom he speaks as having quoted me, read my entire article, he would have found that I have not taken as positive a stand as he credits me with. I have never made the general statement "that metal particles of mercury and cadmium volatilized by the spark do not travel at a higher rate than 100 meters a second." What I have stated is that for a 3 mm spark-gap between *Cd-Hg* electrodes with no (or very small) capacity in circuit there is no motion of the luminous particles as great as 100 cm per second—if we base our judgment upon the four lines measured.

When a medium-sized Leyden jar was inserted in multiple with the spark-gap, there were rather large discrepancies in my measurements. One reason for these discrepancies may be found in the fact that when an image of the spark placed parallel to the slit is focused upon the slit, the lines appear quite uneven—an unevenness which is in part due to differences of intensity, but in part apparently to different breadths of the lines in different parts of the spark-gap. The results therefore were more or less in doubt, but there did not seem to be any Doppler effect—certainly not an amount as great as one would expect from the Schuster-Hemsalech experiment. All of my data, taken together, suggested that "another interpretation may be given to their results" (p. 2 of my paper). The alternative hypothesis was then advanced, viz.: that the curving of the images of the spark-discharge seen in a rotating mirror is due to the propagation of a condition of luminosity in the direction of the discharge.

The argument by means of which Professor Schuster feels that he has established the certainty of the motion of the luminous metal

[1] *Astrophysical Journal*, **25**, 277, May 1907.

particles is one which everyone thinking about the phenomenon, I presume, has constructed for himself. There is no question about metal particles being given off from the electrodes. They must be given off whether capacities are used or not. Let us assume that the curving of the images in the Feddersen experiment is due to the motion of luminous metal particles. Then for the most part only those particles which are freshly driven out during any oscillation of the discharge are luminous, the vapor product of previous oscillations apparently taking but a small part in the discharge. If this diffusion of vapor is necessary for successive oscillations, one would expect that it would be necessary in the prolonged discharge of a coil. Hence I expected to find a Doppler effect in the spark even when capacities were not inserted. Since I found no such effect, with or without capacities, I was led to question the hypothesis of moving luminous particles.

But is the rotating mirror device an accurate analyzer of the spark phenomenon? I do not feel that we can place absolute confidence in the visual evidence it affords. For example, cathode rays streaming through helium excite the molecules of that gas so that they radiate the green-blue line λ 5016. If it were possible to analyze, by a rotating mirror or film, the motion of a rapidly interrupted cathode stream, or if we were to base our conclusions upon the magnetic or electrostatic deflections of this stream, we might conclude, were we ignorant of the nature of the cathode rays, that the stream consisted of particles of helium moving with a great velocity. Indeed, Professor Schuster, in 1890, basing his argument, not upon the above grounds of course, but upon a more or less obvious hypothesis, proved that the negatively electrified particles proceeding from the cathode in a tube filled with nitrogen were atoms of that gas. It remained for other investigators, guilty at the time of bold extrapolation in the construction of a hypothesis, to make the notable discovery of the electron. Evidently infallibility does not always lie on the side of the obvious hypothesis.

The canal stream in helium illustrates my point rather well. I have been unable to find a motion, except a rather small one, of the luminous helium particles. Yet experiments which I have made this

past winter show that magnetic and electrostatic deflections of the stream (with exceptions to be noted in a later paper) are obtained of the same order as those observed for the hydrogen canal stream. Apparently particles of the size of hydrogen atoms, not luminous, are moving through the helium vapor, lighting it up on the way. Evidently the only test whether the luminous particles are in motion or not lies in the presence or absence of the Doppler effect. And if a genuine Doppler effect should be found for the spark-discharge, there would be no question about the luminous particles being in motion.

The review of my article in the last *Beiblätter* (No. 12, 1907) calls my attention to a paper by Hagenbach[1]—a paper which I had entirely overlooked. Hagenbach, using capacity and self-induction in circuit with a spark-gap of 2 to 8 mm between zinc and aluminum, found no Doppler effect when an echelon prism was used as analyzer. With a concave grating he found 0.007 tenth-meters as a superior limit to the Doppler effect. The corresponding velocity is given as 280 meters per second. With zinc and cadmium electrodes he found a smaller velocity.

I have given 100 meters per second as a superior limit to the velocity of the luminous particles in the *Cd-Hg* spark-gap when no capacity was used. With capacity the discrepancies were two or three times as great, so that a superior limit would be about 250 meters per second. The agreement with the results obtained by Hagenbach is very close.

Hagenbach points out that the velocities deduced from his measurements are not at all in accord with those obtained by Schuster and Hunsalech and Schenck. The last observer deduced a velocity of 5,000 meters per second for the luminous particles—a velocity twenty times as great as the possible velocity deduced from the Doppler effect. Hagenbach suggests that the metal vapor may be thrown from the electrodes with great velocity and then brought to a luminous condition by the successive oscillations. "Es ist möglich dass bei jeder Oszillation neuer Metalldampf dazu kommt, doch hauptsächlich wird der schon vorhaudene mitleuchten, ohne wesentliche mechanische Verschiebung." In other words, he believes that, for

[1] *Annalen der Physik*, 13, 362, 1904.

the most part, the luminous particles are not in motion. But he does not account for the curving of the images of the successive oscillations when viewed in a rotating mirror. That phenomenon must be due either to a motion of the luminous particles or to the propagation of a condition of luminosity. His results, as well as my own, are not in accord with the first hypothesis. The rotating mirror experiment does not contradict the second.

G. F. HULL

DARTMOUTH COLLEGE
Hanover, N. H.
July 4, 1907

VENUS AS A LUMINOUS RING

The observation of this rare phase of *Venus*, made by one of us in 1898,[1] was repeated by the other at the conjunction of 1906, the observer's notes being as follows:

On 1906 November 29, $5^h\ 7^m$ G. M. T., *Venus*, being about 1°49′ from the sun's center, was observed with the 5-inch finder of the 23-inch telescope. In moments when the air was steady the complete outline of the planet was distinctly seen. On the side nearest the sun it was bright and easily visible, but on the opposite side it was very faint and could be seen only for a few seconds at a time.

When the complete circle was seen, the space within it always seemed a shade darker than that without. I suspect, however, that this was a subjective effect, as it was not noticed when the fainter part of the ring disappeared through bad seeing. No other marked peculiarities were noticed, though a bright spot was several times suspected in the bright part of the ring.

The sky was very clear and blue. There was a strong wind from the northwest. The seeing was generally poor, but at times it was fair and steady for a few seconds. The whiteness of the field varied noticeably from time to time.

The planet was also observed on November 27 and December 4, and the extent of the crescent measured with a filar micrometer. Bad weather prevented further observations.

Reducing the results by the formulae of the paper already referred to, we obtain the following values for the extent of the twilight arc in the atmosphere of *Venus*.

[1] H. N. Russell, *Astrophysical Journal*, **9**, 284, 1899.

Date, 1907	Observer	Seeing	Apparent Elongation of *Venus* from Sun v	Prolongation of Each Cusp p	Twilight Arc s
Nov. 27.....	H. N. R.	Poor	4° 33′	20°	58′
Nov. 29.....	Z. D.	Fair	1 49	90	>62
Dec. 4.....	H. N. R.	Very bad	7 14	10.5	45

The last value is clearly too small, owing no doubt to the very bad seeing, which would render the delicate extremities of the cusps invisible. The other values agree fairly well with the mean value 70′ found in the earlier discussion.

The ring-phase of *Venus* may perhaps be seen again in 1914, if the atmospheric conditions are very favorable. There will be no other opportunity until 1972.

HENRY NORRIS RUSSELL
ZACCHEUS DANIEL

PRINCETON UNIVERSITY OBSERVATORY
June 3, 1907

A GENERAL INDEX TO THE ASTROPHYSICAL JOURNAL

The preparation of an index to the first twenty-five volumes of this Journal, covering the twelve and one-half years from January, 1895, to June, 1907, is now under consideration. Such an index would doubtless prove of great convenience to the workers in astrophysics and to libraries. The possibility of its publication will depend upon the number of advance orders received. If 200 subscriptions are obtained, the index can probably be issued at a cost of about $1.50; if 300 advance orders should be given, the work will certainly be undertaken, with the expectation of its publication in the autumn of 1907, and the price will probably be somewhat less than $1.50.

All subscribers and librarians who would purchase such an index, if issued, are therefore requested to notify the publishers at once by postcard of the number of copies for which they will subscribe.

Address, The University of Chicago Press, Chicago, Illinois, U. S. A.

NOTICE

The scope of the ASTROPHYSICAL JOURNAL includes all investigations of radiant energy, whether conducted in the observatory or in the laboratory. The subjects to which special attention is given are photographic and visual observations of the heavenly bodies (other than those pertaining to "astronomy of position"); spectroscopic, photometric, bolometric, and radiometric work of all kinds; descriptions of instruments and apparatus used in such investigations; and theoretical papers bearing on any of these subjects.

In the department of *Minor Contributions and Notes* shorter articles will generally be placed and subjects may be discussed which belong to other closely related fields of investigation.

Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right-unless the author requests that the reverse procedure be followed.

Accuracy in the proof is gained by having manuscripts type-written, provided the author carefully examines the sheets and eliminates any errors introduced by the stenographer. It is suggested that the author should retain a carbon or tissue copy of the manuscript, as it is generally necessary to keep the original manuscript at the editorial office until the article is printed.

All drawings should be carefully made with India ink on stiff paper, usually each on a separate sheet, on about double the scale of the engraving desired. Lettering of diagrams will be done in type around the margins of the cut where feasible. Otherwise printed letters should be put in lightly with pencil, to be later impressed with type at the editorial office, or should be pasted on the drawing where required.

Authors will please carefully follow the style of this *Journal* in regard to footnotes and references to journals and society publications.

Authors are particularly requested to employ uniformly the metric units of length and mass; the English equivalents may be added if desired.

If a request is sent *with the manuscript*, one hundred reprint copies of each paper, bound in covers, will be furnished free of charge to the author. Additional copies may be obtained at cost price. No reprints can be sent unless a request for them is received before the JOURNAL goes to press.

The editors do not hold themselves responsible for opinions expressed by contributors.

The ASTROPHYSICAL JOURNAL is published monthly except in February and August. The annual subscription price is $4.00; postage on foreign subscription 75 cents additional. Business communications should be addressed to *The University of Chicago, University Press Division, Chicago, Ill.*

All papers for publication and correspondence relating to contributions should be addressed to *Editors of the* ASTROPHYSICAL JOURNAL, *Yerkes Observatory, Williams Bay, Wisconsin, U. S. A.*